

The British Journal for the Philosophy of Science

VOLUME IX

MAY, 1958

No. 33

THE TWO THESES OF METHODOLOGICAL INDIVIDUALISM ★

LEON J. GOLDSTEIN

I *The Ontological Thesis*

It has become usual in the writings of methodological individualists to suggest that there is but one alternative to their methodological prescription in social science, and this they call 'holism'.¹ Maurice Mandelbaum has sought to counter this by offering a four part classification of types of sociological theories,² thus showing that the alternatives of the individualists is simply an instance of the fallacy of false disjunction. However, it is not at all likely that methodological individualists will find Mandelbaum's paper to the point; it does not deal with the problem that concerns them. I am not at all certain that Watkins, for example, would object to discourse about total societies—the usual meaning of 'holism' and the one with which Mandelbaum deals. He would only insist that such discourse must, at least in principle, be analysable into discourse about individuals and their dispositions, and he does refer approvingly to Ayer's statement that 'the English State . . . is a logical construction out of individual people'.³ We need not be concerned with what Ayer here means by

★ Received 31. xii. 57

¹ Cf. J. W. N. Watkins, 'Historical Explanation in the Social Sciences', this *Journal*, 1957, 8, 104-117. In view of the many connotations of 'holism', I deem it regrettable that E. A. Gellner, in his otherwise admirable paper, 'Explanation in History', *Aristotelian Society Supplementary Volume XXX*, 1956, pp. 157-176, continues the practice of using 'holist' for everything non-individualistic. I know of no really suitable term, but I much prefer the more cumbersome 'non-individualistic social science' to 'holism'. (And we shall see that they are by no means synonymous terms.)

² 'Societal Laws', this *Journal*, 1957, 8, 211-224

³ *Language, Truth and Logic*, London, 2nd edn., 1950, p. 63; cited in Watkins, 'Ideal Types and Historical Explanation', in Feigl and Brodbeck, eds., *Readings in the Philosophy of Science*, New York, 1953, pp. 723-743, 730; cf. p. 729 n. 18.

logical constructions, for I want only to show, since the English state is surely a social whole, that Watkins cannot be said to oppose a social whole. In this example, a holist is one who denies Ayer's assertion, not of necessity one who is interested in the English state.

According to Watkins, 'If methodological individualism means that human beings are the only moving agents in history, and if sociological holism means that some superhuman agents or factors are supposed to be at work in history, then these two alternatives are exhaustive'.¹ He further says, 'The central assumption of the individualistic position . . . is that no social tendency exists which could not be altered *if* the individuals concerned both wanted to alter it and possessed the appropriate information This assumption could also be expressed by saying that no social tendency is somehow imposed on human beings "from above" (or "from below")'.² Holists, it appears, are those who posit non-human entities which in some unexplained way are supposed to determine what happens to men. Human history, on this view, would, paradoxically, not be the history of men and their affairs, but rather of that entity or set of entities that impose themselves upon them. Holists may well be interested in entities that are total, that is, they may conceive their holistic entity to be a unitary sort of thing, and perhaps Hegel's *Weltgeist* is an instance of this. But it is not impossible that holists of more modest pretension could be concerned with entities of a less inclusive nature. Perhaps the 'basic patterns' of the anthropologist Kroeber are such lesser entities,³ and it may be argued that their logic is rather like that of the *logoi spermatikoi* of Hellenistic thought.

If methodological individualism is the thesis that denies the existence of these non-human entities, it is clear that neither Mandelbaum, Gellner, in the paper cited above in a note, nor I, in my earlier paper on this subject,⁴ may be said to have opposed it. Thus, one may wonder what the controversy is all about. The fact of the matter is, however,

¹ Op. cit., p. 106

² Ibid., p. 107; his italics. I agree with the main point of this statement and would add only that included in the 'appropriate information' may well be non-individualistic social laws. Dr Israel Scheffler, of Harvard University, observes that what Watkins says about social tendencies is equally applicable to furniture and machines, any of which could be altered if the individuals concerned both wanted to and had suitable knowledge.

³ A. L. Kroeber, *The Nature of Culture*, Chicago, 1952; cf. chs. 5, 9 and 23

⁴ 'The Inadequacy of the Principle of Methodological Individualism', *Journal of Philosophy*, 1956, 53, 801-813

TWO THESES OF METHODOLOGICAL INDIVIDUALISM

that under the rubric 'methodological individualism' Watkins has subsumed two positions. The one we have been dealing with is not a methodological position at all. This non-methodological—ontological—version of the principle of methodological individualism is that doctrine which denies the existence of certain alleged entities. The other, more truly methodological thesis is the one which claims that all explanation in social science must, in the end, be reduced to individual dispositions. I shall attempt to deal with what this means in the following section of the paper. For the present I wish to discuss further this ontological thesis. It is not enough, it seems to me, simply to separate the two individualisms and observe that it is quite likely that in the ontological version we discover a point of agreement for most of us who have been interested in this matter. For this, I have two reasons. The first is that the bulk of the rhetorical force of the arguments that are supposed to be in defence of the more methodological of the individualist theses comes from confusing its denial with denial of the ontological thesis, and the fact that for most of us the ontological holist thesis is untenable. The suggestion that non-individualistic social science has evil consequences for freedom and morality has never been made out. Instead, we are supposed to wonder just what can possibly be intended when we talk of freedom and morality in a social world exhaustively determined by non-human entities which impose its history upon man.

My second reason for being unwilling to end discussion of the ontological version is that we are often told that holism is inextricably bound up with another methodological evil, historicism,¹ and here, too, holism and non-individualistic social science are confused. The thesis of historicism is that nothing men do makes a difference; all that will happen happens of necessity given the nature of history, or society, or the dialectic, or the *Weltgeist*. But here 'history', 'society', 'the dialectic' or 'the *Weltgeist*' serve as possible names of a holistic entity, the existence of which both Watkins and I agree in denying. Historicism is, then, an appurtenance of holism, and neither has to do with the claim of some of us that social science must be non-individualistic. Methodological individualists have never shown that to deny the assertion that all explanation in social science must be in terms of

¹ In Popper's sense of historicism, not Mannheim's. By 'historicism' Mannheim means the socio-historical determination of thought, which need not commit him to the view that in history we have the necessary actualisation of what was potential in earlier stages.

individual dispositions entails historicism, and man's consequent helplessness, and I hope to suggest that this is not the case. If they think they have it is because they have not distinguished between their methodological and their non-methodological doctrines.

Whether or not one believes that non-individualism in social science can be defended without the risk of ontological error, it is surely the case that some social scientists have been aware of the distinctions that I have been trying to make above. Thus, Simmel says,

No matter whether we consider the group that exists irrespective of its individual members a fiction or a reality, in order to understand certain facts one must treat it as if it actually did have its own life, and laws, and other characteristics. And if one is to justify the sociological standpoint, it is precisely the difference between these characteristics and those of the individual existence that one must clarify.¹

In the same way, Durkheim remarks,

So there is some superficiality about attacking our conception as scholastic and reproaching it for assigning to social phenomena a foundation in some vital principle or other of a new sort. We refuse to accept that these phenomena have as a substratum the conscience of the individual, we assign them another; that formed by all the individual consciences in union and combination Nothing is more reasonable, then, than this proposition at which such offense has been taken; that a belief or social practice may exist independently of its individual expressions. We clearly did not imply by this that society can exist without individuals, an obvious absurdity we might have been spared having attributed to us . . .²

In sum, there are any number of people who have given thought to this matter who would say with Professor Ginsberg, 'To assign characteristics to groups is by no means the same as to consider them as entities which exist independently of the individuals which compose them'.³

All of these passages show that their authors feel no need of the belief in hypostatic social entities, yet they insist that explanation in social science need not, and perhaps cannot, be individualistic. If methodological individualists want to claim that this is not a consistent set of beliefs, they must show that the rejection of what I refer to as their methodological thesis entails or strongly supports the ontological position we have discovered that Watkins intends by the term 'holism'.

¹ *The Sociology of Georg Simmel*, tr. and ed. by Kurt Wolff, Glencoe, Ill., 1950, p. 26

² *Suicide*, tr. by Spaulding and Simpson, Glencoe, Ill., 1951, pp. 319 sq.

³ *On the Diversity of Morals*, Melbourne, London, Toronto, 1956, p. 152

TWO THESES OF METHODOLOGICAL INDIVIDUALISM

But this has never been done, perhaps never even attempted. And I dare say it never will be so long as methodological individualists do not trouble to distinguish between the two kinds of individualism they support.

That the rejection of the methodological thesis does not entail or support holism¹ is suggested by Durkheim's referring to the community of consciousness or sentiment as a consequence of individuals living together. There are many such passages in his writings, and his failure to take pains in expressing himself on the point has led his critics to saddle him with belief in something called 'the group mind' or an actually existing collective. We have seen that he denies that this was his intention, but that apart, he is certainly not required to affirm it. It seems to me that a possible construction of Durkheim's words would have him advocating a point of view that may be named 'sociological emergence'. This would claim that, for whatever reasons, when human beings live together, share common experiences, and so forth, there emerge common sentiments and modes of representation² which would never have arisen apart from group life and which cannot be analysed into the bio-psychological characteristics of unsocialised individuals. A view such as this does not require the support of holism. Indeed, it is entirely incompatible with that doctrine as it has been characterised by Watkins. According to Durkheim, collective sentiments emerge out of the social intercourse of human beings, but the holist view is that such sentiments are imposed upon us by non-human entities, the true source of human history. Furthermore, since on the view we are considering what men do does make a difference, there being no emergent sentiments unless there are human beings sharing common experiences, historicism is equally incompatible with it. Presumably, the kinds of sentiments, values, modes of representation, and so on, that emerge depend upon the sorts of experiences that are shared, and man is not treated as if he were caught in the grip of some strange and monstrous being whose necessary development determines the course of history.

¹ That is, the decision to reject the methodological thesis leaves one uncommitted so far as the ontological thesis is concerned, though it seems most reasonable that anyone who agrees with Watkins concerning the former ought to agree concerning the latter as well. Logically, this isn't necessary, for one, being so-minded, could develop a holism in which the holistic entity had about as much to do as a deistic god after the creation.

² Cf. his 'Individual and Collective Representations', in *Sociology and Philosophy*, London, 1953, pp. 1-34

Sociological emergence is not, to be sure, a sociological theory. It does not in any way explain how it is that social institutions emerge. Nor does it have any specifically testable consequences. But in these respects it is not unlike the methodological thesis of methodological individualism. This, too, explains nothing, nor does it purport to explain anything. No sociological theory need make explicit reference to sociological emergence; its usefulness is of another sort. When methodological individualists assail this or that theory as holistic, when in fact it simply uses concepts that are not reducible to individual dispositions, its defenders have always the possibility of pointing to methodological emergence or some variation of it. That is, since the nature of the criticism levelled against the theory is ontological rather than methodological, sociological emergence offers a way of meeting it. It affirms that social scientists may develop non-individualistic theories without being holists. And it has the further advantage of forcing the methodological individualists to defend their methodological thesis on methodological grounds. If non-individualistic social science does not commit untoward ontological sins, the methodological individualists are required to find better grounds for its rejection. The doctrine that all explanation in social science is ultimately in terms of individual dispositions is not established, indeed, in no way supported, by the untenability of holism.

2 *Individual and Anonymous Dispositions*

There is, as we have seen, a properly methodological thesis included under the rubric 'methodological individualism', namely, the claim that in social science all explanation is to be individualistic. This sometimes means that we are required to carry each explanation back, step by step, until we have transformed sociological explanations into psychological ones.¹ Very often in his writings Watkins talks about *individual dispositions*, and perhaps there is in this mode of speech a reference to the ontological matters discussed above. We are said to be interested in the dispositions of individual human beings and not those of societies or other holistic entities. But this apart, it will be interesting to discover what sorts of thing pass muster, in his view, as individual

¹ 'From this truism I infer the methodological principle which underlies this paper, namely, that the social scientist can continue searching for explanations of a social phenomenon until he has reduced it to psychological terms' (Watkins, this *Journal*, 1952, 3, 28 sq.).

TWO THESES OF METHODOLOGICAL INDIVIDUALISM

dispositions. Fortunately, he has himself provided us with a number of examples, and the following quotations may serve as an adequate sample.

What Smith actually showed was that individuals in competitive economic situations are led by nothing but their *personal dispositions* to promote unintentionally the public interest . . .¹

Suppose that it is established that Huguenot traders were relatively prosperous in seventeenth-century France and that this is explained in terms of a wide-spread disposition among them (a disposition for which there is independent evidence) to plough back into their businesses a larger proportion of their profits than was customary among their Catholic competitors . . .²

Finally, in a letter to me and by way of attempted refutation of my claim³ that the principle of methodological individualism is not adequate to deal with such social scientific work as the theory of kinship nomenclature in G. P. Murdock's *Social Structure*, Watkins writes,

What gives a kinship system its stability and distinctive characteristics is, as you say, certain *rules* about marriage, inheritance, residence, etc., which govern the behaviour of each member to his "relations". But a local rule of inheritance, say, is simply a disposition shared by pretty well everybody in the locality to deal with the property of dead persons in a determinate way. I conclude that there is no difficulty in the idea of an anthropological explanation of the characteristics of a kinship system in terms of disposition and beliefs.

It may be noted that Watkins's closing remarks in the material last quoted indicate that he has misconstrued the point of my paper. I did not there challenge the view that societies or parts of them could be characterised in individualistic terms, though I must confess that I have come more and more to doubt that even this is possible,⁴ but rather the view that sociocultural theory must be individualistic. The problem to which Murdock addresses himself is that of accounting for how it is that systems of kinship develop and change. This is rather different from merely characterising or describing any given such system, and even if one believes that the characterisation may be done in individualistic terms, rather than in non-reducible institutional terms, the status

¹ 'Ideal Types and Historical Explanation', p. 730

² Op. cit., p. III

³ Goldstein, op. cit., pp. 803-808

⁴ Such characterisations would tend to blur the distinction between the rôles of individuals and the domains of institutions, to use the apt expression of Mr Harold Weisberg. Cf. Mandelbaum, 'Societal Facts', *British Journal of Sociology*, 1955, 6, 305-317

of the sociological laws which govern the development and change of such systems may perhaps require separate determination.

But putting aside this distinction between characterising what I have called the 'synchronic social now', that is, the institution under study during that period of time within which it remains more or less stable,¹ and characterising the laws that govern it, we have to consider what can be meant by calling the dispositions cited in the above passages 'individual dispositions'. It has been made amply clear by Watkins that he wishes to include as individualistic not only the dispositions of specific people about whom we may have factual knowledge, but also the dispositions of anonymous people.² Detailed individualistic explaining may well be an ideal for which to strive, but in most instances it is not a realisable goal, and there seems to be no point in virtually ruling the social sciences out of business by insisting upon it. All of the examples quoted are instances of these so-called anonymous dispositions. They are not only the dispositions of specific individuals with whom the social scientist may be concerned, but if his explanation is adequate we should be able to discover in many other people, concerning whom factual evidence may yet become available, the same characteristics or dispositions described.

I do not doubt that it was individual Huguenots who ploughed back their profits into their respective businesses, but it is not at all obvious that ploughing back—or, for that matter, being a Huguenot—is anything but a social or sociocultural characteristic. Properly to understand this kind of behaviour I am required to know something about the economic conditions of seventeenth-century France, the kinds of business enterprises that flourished, which of them, if any, were especially noted for the concentration in them of Huguenots, and so forth. Presumably I would wish to know other things, not of an economic nature, which bear upon such matters; e.g. if the governments of European countries were wont to interfere in economic affairs, this could be an important bit of relevant information. And thus a more or less full picture of Huguenot economic activities could be developed. But in all this no mention is made of any individual; no concern is felt for the psychological characteristics of anybody. We do not, to be sure, deny, as Watkins thinks we must, that all the time individual human beings are making decisions to buy and sell, to

¹ Op. cit., pp. 809 sq.

² Watkins, 'Methodological Individualism: a Reply', *Philosophy of Science*, 1955, 22, 58-62, p. 62

TWO THESES OF METHODOLOGICAL INDIVIDUALISM

manufacture in greater or lesser quantity, and to export or import this or that. But it is not obvious that we need be concerned with them in reconstructing the economic conditions of seventeenth-century France.

Similar observations may be made about the characterising of the social and kinship system of a people. Specific people are related in specific ways, but the rules governing the determination of these relations are expressed—by the ethnographer even when not fully understood by the people themselves—in ways that do not make reference to individuals or to psychological characteristics. For the most part, people are born into their kinship relationships, and it seems entirely a reversal of actual fact to say that such relations 'are the product of peoples *attitudes* to each other, though these are partly determined by their *beliefs* about their biological relations'.¹ It seems more reasonable to say that for the most part the proper attitudes towards one's various kin are cultivated during the enculturation process.

The point here is that the kinds of dispositions to be found in people of any given type are socially induced dispositions. It seems odd to talk about widely recurring dispositions among Huguenot entrepreneurs and not to wonder about the coincidence of the recurrence in just this group. It was, to be sure, individual Huguenots who successfully competed in the business world of the seventeenth century. But this was presumably because the Huguenot upbringing or enculturation produced people who were adept at this sort of thing. These were people who could operate effectively within the socioeconomic framework of the time.

It seems to me that methodological individualism makes sense only if it can demonstrate the relevance of psychological characteristics in explaining and describing social institutions, and hence I am unable to agree that anonymous dispositions are individualistic. Anonymous dispositions convey no information about anyone in particular, but individuals are individuals in particular. I am not, of course, saying that no individual is disposed to behave in such-and-such way, where 'such-and-such' refers to some anonymous disposition. Bank tellers, to use Mandelbaum's example, are disposed to behave in ways suitable to their occupation, but one cannot explain banking institutions in terms of such dispositions to behave. It is the institutional framework of

¹ From a letter from Watkins to me; it immediately precedes the long sentence quoted in the text above

banking which fixes the manner deemed suitable for tellers. And when some individual bank teller finally achieves his goal and is promoted to a junior vice presidency, his mode of behaviour changes. But it seems rather odd to say that he has now acquired a new set of personal dispositions and mean thereby that his psychological nature has undergone some change.¹ He has merely stepped into a new rôle and is now prepared to act in ways suitable to his new station in life.

In insisting upon the sociological character of the concepts with which we describe institutions and the social nature of so-called anonymous dispositions, it is not my intention to say that individual people make no difference. Likewise, such insistence need not lead to any undesirable ontological results. There are people and they do make a difference, and yet they are sufficiently similar—at least so we are told by many responsible students of social science²—that laws of social change may be formulated. Not, to be sure, historicistic laws of development, but laws in which the relevant variables are specified and the nature of the change made determinant without recourse to oracular pronouncements that obfuscate more than they clarify. I have suggested elsewhere³ that Murdock's theory of kinship systems is an example of this sort of theorising. This theory proclaims no necessary successions; it requires—as does any theory—only that if it is accepted as true or approximately so, then given the theoretically necessary and sufficient conditions for the development of any kinship system of determinate type, then we may reasonably expect an instance of that type to develop.

Murdock himself actually thinks that behavioural psychology and psychoanalysis are relevant to his theory,⁴ but in actual fact these play no logical rôle in the formulation of the theory or the derivation of its results.⁵ I suspect that his interest in this may be part of a desire to avoid the reification of societies, holism as we now understand it. It is precisely this concern which leads us to sociological emergence, and

¹ Even if it were not odd it would not support the individualistic position inasmuch as it would suggest the dependence of personal dispositions upon non-personal institutional factors.

² This is implicit in a good deal of work that is done in the social sciences and has been explicitly insisted upon by a good many of those who work in those fields, especially by anthropologists. The well known comparative approach of anthropology would seem to make little sense if this were not believed.

³ *Op. cit.*, pp. 803-808

⁴ *Social Structure*, New York, 1949, p. xvi

⁵ Goldstein, *op. cit.*, pp. 807 sq.

TWO THESES OF METHODOLOGICAL INDIVIDUALISM

it may well be that if one wanted to determine in specific detail the dynamics of emergence, so to speak, it would be necessary to make use of psychological theory. But I am not able to see that we are therefore required to reduce the social to the psychological any more than we do the psychological to other so-called levels of reality. Nor does it follow that because concern with the actual dynamics of emergence brings us to psychology, that psychological theory has any function at all in the formulation of sociological theories.

The notion that we can talk about the dispositions of anonymous people seems to me somewhat strange. I know what it means to characterise particular people in terms of their particular dispositions. If I am told that John is lazy, I understand that he is disposed to avoid work; if told that he is thrifty, I know him to be a careful man with a dollar (or a pound, as the case may be). These are personal dispositions, though the sort of behaviour characteristic of the expression of such dispositions seems to be cultural, not psychological. But it is one thing to attribute such tendencies to John, quite another to so-called anonymous individuals. I venture to suggest that wherever Watkins talks about anonymous dispositions or the dispositions of anonymous individuals he is simply attempting to talk about non-individual characteristics of societies or part of societies, or of socially induced ways of behaving, without being explicit about it. Anonymous individuals are referred to as a way of avoiding holism, but we have seen that this may be done in other ways. There are specific individuals and characteristics of social phenomena; each of these raises theoretical questions of its own, and it is the business of psychology and social science to deal respectively with them. But there is no science of the anonymous. What we have are not the characteristic dispositions of people we don't know, but the social behaviour of people in given situations quite apart from their personal dispositions.

University of Maryland
C.S.C.S., Atlantic Division
U.S.A.

THEORIES, DICTIONARIES, AND OBSERVATION ★

MARY B. HESSE

I

ACCORDING to a widely accepted account of the logic of scientific theories, statements of a theory may not be directly testable, since they may mention concepts which are not empirically observed, but it is necessary that some of the logical consequences of the theory shall be interpretable in terms of observation statements which are directly testable, and that no such observation statements shall have been in fact falsified. A distinction is commonly made between direct observation and theoretical inference in such a way as to make it appear that observation statements constitute a basic empirical language which is independent of all scientific theory, and which can thus function as the ultimate court of appeal for the truth of the theory. I shall first examine a few examples of this distinction, and then try to show that if the observation statements are to be tests of the theory, the distinction cannot be maintained.

(i) In the account of N. R. Campbell,¹ the empirical language consists of statements of laws of nature which assert uniform association or functional relationship between 'concepts', which, although not 'simple judgements of sensation' (p. 45), because their definition often depends on the truth of some other law of nature, nevertheless seem to be directly measurable quantities, since Campbell's examples include 'length, weight, period, electrical current, . . . temperature' (p. 105), and not, for example, curvature of space or the diameter of gas molecules. Laws of nature can be proved 'by direct experiment' (p. 130), or, at least, 'though they may not always be capable of being proved by experiment, are always capable of being disproved by it' (p. 131), and 'it must be possible to determine, apart from all knowledge of the theory, whether certain propositions involving these ideas [the laws] are true or false' (p. 122).

★ Parts 1, 2, and 3 were read at the second annual conference of the Philosophy of Science Group, Nottingham, 21st September 1957.

¹ *Physics, the Elements*, Cambridge, 1920

THEORIES, DICTIONARIES, AND OBSERVATION

Hypotheses, on the other hand, according to Campbell, have a different status from laws, since they have not this direct relation to experiment, but they are connected to laws by means of a 'dictionary' (p. 122), which 'interprets' hypothetical ideas into concepts, so that the laws allegedly relating these can be tested.

(ii) F. P. Ramsey,¹ following Campbell, speaks of a *primary system* containing all the terms and propositions of the universe of discourse in question (in his examples these are sense-data), and a *secondary*, or theoretical, system, related to the primary system by means of a dictionary which defines 'the functions of the primary system in terms of those of the secondary system' (p. 215). Many propositions of the secondary system will not have 'any *direct* meaning' (p. 235, Ramsey's italics).

(iii) Professor Ayer² speaks of 'observation-statements' which record 'an actual or possible observation' (p. 11), and regards a statement as *directly verifiable* if it is an observation statement, or 'is such that in conjunction with one or more observation-statements it entails at least one observation-statement which is not deducible from these other premises alone' (p. 13). Directly verifiable statements are distinguished from those that are *indirectly verifiable* but which appear in scientific theories although they do not designate anything observable. A characteristic of indirectly verifiable statements in science is that they are connected with directly verifiable statements by means of a dictionary which 'can be regarded as analytic'.

(iv) Professor Braithwaite³ distinguishes between 'propositions about observable entities', for example 'flashes of light or pointer-readings of a measuring instrument' and those containing *theoretical concepts*: 'fields of force, wave-functions, electrons' (p. 51). Direct meaning is given to the first kind of proposition, indirect meaning to the second.

(v) Professor Woodger⁴ divides biological statements into (1) observation records and (2) theoretical statements. The latter all go beyond what can be directly observed and therefore cannot be verified, but their consequences are testable, and it is upon this consequence-structure that they depend. 'It is with the help of this relation [of consequence] that the testing of hypotheses is possible, and this is

¹ *The Foundations of Mathematics*, London, 1931

² *Language, Truth and Logic*, 2nd ed., London, 1946

³ *Scientific Explanation*, Cambridge, 1953

⁴ *Biology and Language*, Cambridge, 1952, p. 12

independent of the meaning of the descriptive signs which occur in the *high-level* hypotheses. For this reason it is quite unnecessary that the descriptive signs in such statements should have "meaning" in the sense that we should be able to represent them as objects which we can see or imagine' (p. 58).

The view of science implied by these five writers agrees in the following points:

(a) There is some set of statements whose truth or falsity is known directly by observation, although it is not necessarily known indubitably. It is assumed that what these statements are is sufficiently indicated by giving examples and counter-examples.

In order to avoid further confusion of terminology, I shall call these statements *phenomenal*, a word which is neutral as between Campbell's laws of nature asserting relationships between 'concepts', Ramsey's 'primary system', Ayer's 'observation-statements', Braithwaite's 'propositions about observable entities', and Woodger's 'observation records'. I wish to avoid use of the word 'observable' at this stage for reasons which will be apparent later, and the word 'phenomena' has the merit of having been used by Newton and by Kant in senses not too far removed from what is here intended. The word is not, however, to be taken as carrying any overtones of phenomenalism, if by that is meant a doctrine according to which all we directly know are sense-data. Phenomenal statements are intended to cover all ordinary indicative sentences descriptive of physical objects, including some general sentences.

(b) The five views agree that there are statements in science which do not fall into the phenomenal class. I shall call these *theoretical statements*.

(c) The views further agree that the meaning and criteria of truth of phenomenal statements are independent of those of theoretical statements.

(d) It is also said that the meaning of theoretical statements is dependent upon a dictionary which translates some of them into phenomenal statements.¹

I shall call the view of science expressed in these four points the 'dictionary theory'. I suggest that the motive for its adoption was twofold: firstly, to contrast the clarity and certainty of empirical tests with the tentative nature of theories, and secondly to show nevertheless how theories could be unambiguously tested and hence

¹ Woodger does not use the word 'dictionary', but the idea is implied by his discussion of the meaning of hypotheses quoted above.

THEORIES, DICTIONARIES, AND OBSERVATION

given meaning by experiment, and so distinguished from speculative metaphysics for which no such tests were available. What I wish to show here is that if phenomenal statements are to be tests of theories, then their meaning cannot be entirely independent of that of the theories, and that consequently the function of the dictionary has been misconceived in these accounts of the relation between theory and experiment. Then I shall discuss the use of the word 'observable' and its cognates in specifically scientific contexts.

2

It is necessary first to define phenomenal statements a little more closely, since it is generally agreed that they cannot be characterised by their certainty or incorrigibility. It is easy to see that they cannot be *logically* certain, for there is no necessary logical relation between the occurrence of a single public event and the written or spoken report of it, much less between a series of allegedly similar events and the general phenomenal statement which asserts that under these given circumstances, such and such always occurs. The phenomenal statements of science are almost always general in this way: they do not describe what happened on a particular occasion to a particular observer, but what always happens and will happen on sufficiently similar occasions to all normal observers. Such statements cannot be claimed to be incorrigible, but they are characterised by a high degree of *empirical* certainty (for the possibility of mistakes is minimised by experimental techniques), and of invariance with respect to repeated tests and different observers.

What is not so often noticed, however, is that the claim that phenomenal statements are unambiguous cannot be maintained either. This claim is implied in the assertion that the meaning of the statements is independent of any theories, and it relies upon the existence of a phenomenal language which involves no technical terms and which is understood by everyone who speaks the natural language in question, and in which the phenomena upon which scientific theory ultimately rests can be unambiguously described. Now it is perfectly true that there is such a language, containing only the common-sense descriptions of ordinary objects and processes, and also, if necessary, descriptions of what come near to sense-data in dealing with unfamiliar objects and processes encountered in scientific observation. All scientific observations can be described in such terms, mentioning apparatus whose

specification cannot involve any knowledge of theory, since it has in any case to be understood in the workshop, and describing coincidences of pointers with scales, and clicks, flashes, bands of coloured light, etc. No doubt it is impossible to make precise this notion of being commonly understood in the language, for what is commonly understood among one group of people may not be so among others, but I shall assume, as do most of the exponents of the dictionary theory, that it is possible to recognise a statement as being phenomenal in this sense, and that if any alleged phenomenal statement is challenged because it contains some technical phrase such as 'current in the wire', it is always possible to find an acceptable translation into phenomenal terms, such as 'the position of the pointer on that scale'.

Phenomenal statements, then, differ from ordinary empirical descriptions only in having the characteristics of certainty, publicity, and repeatability to a higher degree than is usually possible or necessary. Normally descriptions of experiments are not given in this form because to do so would be tedious and unnecessary, but if necessary, for instance when radical revisions of a theory are in progress, most descriptions could in principle be reduced to phenomenal statements as here defined. The dictionary theory asserts, however, that such phenomenal statements function as tests of theoretical statements in the sense of being derivable from the latter, and being determined as to their truth or falsity by empirical considerations which are independent of theory. I now wish to show that if phenomenal statements are to be tests of a theory, they cannot be independent of the theory as here suggested. The conditions given for phenomenal statements are not sufficient for them to have scientific significance, and if they are to have such significance, there must be connections of meaning between them at a higher than common-sense level, and therefore the condition of complete theoretical independence between them must be dropped. Physics can be reduced to phenomenal statements if these connections of meaning are ignored, but such reduction does not give a full account of the observational basis of physics, for it cannot explain why just these statements are chosen and not others.

3

A phenomenal statement can never test a theory alone, but only a theory together with its dictionary interpretation into this particular phenomenal statement. Consider the stick apparently bent when

immersed in water. The situation can be described in detail in a phenomenal statement using only words universally understood: 'surface', 'sun', 'air', 'water of given salinity' (this last one can be further unpacked), and so on. Suppose we set up a theoretical deductive scheme mentioning light rays and Snell's law, and derive from it an angular measure. With the appropriate interpretations 'source of light' into 'sun', 'media of different refractive index', into 'air and water', and so on, we can identify this theoretical angle with our angle measurement, and assert that the phenomenal statement confirms the theory of refraction together with this interpretation. But how do we know that this is an interpretation which is relevant to this theory? If in the equation expressing Snell's law, $\sin \alpha / \sin \beta = \mu$, α and β were simply undetermined mathematical symbols, they might be interpreted in an indefinite number of different ways, some of which might be shown to be true by observation. They might for example be the angles between the Pole Star and Mars and Venus respectively at midnight on certain given dates; why would not this be a confirmation of the formalism we have mistakenly called the wave theory of light? Conversely, why can we not invent any theory at all which leads deductively to the equation $\sin \theta / \sin \phi = \mu$, and interpret θ and ϕ as the angles of refraction, and take our refraction experiment as confirmation of this theory?

The answer is of course that the possible interpretations of α and β are already circumscribed by the theory, which itself talks about light rays and media of different refractive index, and that we already know the kind of observation which will be relevant to this theory. If the theory is to be acceptable the equation $\sin \alpha / \sin \beta = \mu$ must also be interpretable into other relevant observations besides that of the bent stick. Suppose we take a burning glass and use it to burn a piece of paper. The concentration of heat at a given point will only be a confirmation of the theory of refraction used to explain the bent stick if it is assumed that 'media of different refractive index' may be interpreted into 'air and glass' as well as 'air and water', 'interface' may be 'curved surface' as well as 'horizontal surface', and 'radiation' may be 'heat' as well as 'light'. That these are relevant interpretations cannot be given in the phenomenal statements alone, because by definition these cannot contain theoretical overtones. It may be objected that no theory is required to see that the phenomena of bent sticks and burning glasses are connected, and that it is obvious to common-sense that both need accounting for in terms of the same

theory. This may be true, although it clearly depends on the extent to which 'common-sense' has been permeated by scientific notions. But there are other examples in which it is certainly not true. To a layman entering an atomic physics laboratory it would not be at all obvious that certain pairs of scintillations over here are connected with the presence of a nuclear reactor over there, and that they confirm a theory which explains why certain other scintillations near the reactor have the character they have. But to a physicist they count as the detection of neutrinos whose presence was suspected because of a loss of energy in certain decay processes in the reactor. The mere report of scintillations expressed in a phenomenal statement cannot proclaim its own relevance to this theory, nor its connection with phenomena on the other side of the laboratory; the observation becomes a confirmation of the theory only when it is interpreted into language about fundamental particles, and this can certainly not be done in phenomenal statements as they have been defined.

It might be suggested at this point that all the inferences involved in interpreting the observation as a test of the theory could be broken down into phenomenal statements together with such elementary links between them as that involved in seeing that two simple optical phenomena are connected, as in the first example. But this is just what is done when the neutrino theory is *taught*, and it is a process which involves teaching the whole of physics. If all this is necessary before a phenomenal statement can be said to be a test of the theory, then it certainly cannot be maintained that the testing function of the statement is independent of theory.

There is a view of theories which might seem to avoid the objections here brought against the simple dictionary theory, that is, that the function of phenomenal statements is not to *test* theories, but merely to determine their *scope*.¹ Thus it might be said that the burning-glass experiment by itself is not a test of the wave theory of light, because whatever the result of the experiment, the theory could be saved by appropriate, and perhaps very limited, interpretations of its symbols. Similarly if the expected scintillations were not observed, this would show only that the scope of the neutrino theory did not extend to these particular experiments. But it would be very queer to say this. Burning glasses and these kinds of scintillation are just what the theories respectively are about, and if the obvious interpretations, which are determined within the theory, do not lead to confirmatory observations,

¹ Cf. S. Toulmin, *The Philosophy of Science*, London, 1953, p. 63

THEORIES, DICTIONARIES, AND OBSERVATION

then some further theoretical explanation of this fact is required, or else the theory is abandoned. There are of course cases where it is the scope of a theory that has to be determined by observation, for example Boyle's law is confirmed only over a certain range of temperature and pressure, but then we look for a more general theory to explain the deviations from Boyle's law, and this theory has in turn to be tested. It is not profitable to confuse this kind of example with the others where a theory is properly said to be tested.

Or again, it may be suggested that scientific theories are just abstract correlations of some phenomenal statements or other, no matter what these phenomenal statements are; that we can, as it were, parcel together any collection of phenomenal statements from many diverse areas of experience, and that so long as we can find a correlating theory, this will be a satisfactory piece of scientific explanation. This is not as implausible as it sounds, for, as Professor Dingle has pointed out, something like it happened in seventeenth-century physics when study of Aristotelian substances with diverse qualities was abandoned in favour of the study of the primary qualities common to all substances. Phenomena involving mass and motion, however diverse in other ways, were parcelled together under the theory of mechanics, while complex individual substances were divided into various aspects and became subject to many different kinds of theory. That many different parcellings of phenomena for the purposes of theoretical explanation are conceivable does not however show that these parcellings can be quite arbitrary. The fact that one can, as in this example, describe the principles upon which the new parcelling was done, namely that primary qualities were distinguished from secondary, shows that it was *not* arbitrary. That it can never be quite arbitrary and remain scientific is sufficiently shown by considering that a theory developed *ad hoc* as a correlation of a completely diverse collection of phenomenal statements could never be predictive, for what would decide what kinds of phenomena the theory was supposed to give predictions about? One could only determine the scope of the theory in relation to new phenomena by making observations and doing experiments at random; some of them might be fitted into the theory and others not, but nothing could count against the theory and the theory would have no predictive content. Such a procedure is contrary to the practice of science.

It may therefore be concluded that if the meaning of phenomenal statements is independent of theory, then they do not test the theory.

If this is the case, is there any justification at all for the two-tiered structure of statements described by the dictionary theory? It is already admitted by most writers that the meaning of phenomenal statements is contextual, and if the context includes certain theoretical ideas, introduced by the use of even the most simple technical words in phenomenal statements, must one not characterise the situation quite differently, and say that theoretical language is *richer* than phenomenal language and never even in principle translatable into it?

To interpret an experiment directly in theoretical terms so that it can be a test of the theory is always to say more than the corresponding phenomenal statements would say, because such interpretation carries with it natural expectations about possible but so far unobserved behaviour which the scientist has to *learn*, just as the child learns the contextual overtones of ordinary language. Suppose an observation is reported in the following terms: 'if in a Wilson cloud-experiment the quantum of scattered X-rays produces a photo-electron in the chamber, then a line drawn from the beginning of the recoil track to the beginning of the track of the photo-electron gives the direction of the quantum after scattering. It was therefore possible to test the truth of the equation

$$\left(1 + \frac{h\nu}{mc^2}\right) \tan \phi/2 = \cot \theta$$

which connects the directions of the scattered quantum and the recoil electron.'¹ This cannot be translated without remainder into a phenomenal description of the cloud-chamber and an apparatus producing X-rays, together with the statement that there are two finite non-intersecting white lines and the observer has drawn a third line joining one end of one to one end of the other, and measured the angles between the lines. This is the whole phenomenal account of the experiment, and clearly it means nothing and could test nothing, because it does not state its relevance to anything. θ and ϕ as measured on the photograph may satisfy the equation, but they cannot be known to be the θ and ϕ which are mentioned in the theory from which the equation was derived unless the experiment is interpreted in terms of tracks of photo-electrons and so on. The relevance of the experiment is irreducibly theoretical.

Where one language is richer in associations than another its significance cannot be fully represented in the other by means of a dictionary translation. The correct analogy for the relation between

¹ E. Whittaker, *History of Theories of the Aether*, II, Edinburgh, 1953, p. 212

THEORIES, DICTIONARIES, AND OBSERVATION

theoretical and phenomenal languages is not the relation between simple sentences in English and in French, but the translation of poetry into pedestrian prose; or, to look at it the other way round, the phenomenal description of an experiment has a relation to the scientist's theoretical description which is similar to that between Holingshed and Shakespeare. The hierarchy of statements in science is like the gradation from prose to poetry in being characterised by varying degrees of imagination, but it is also unlike in being correlated approximately with degrees of factual certainty.

There is, however, a place where talk about dictionaries is in order, and this can be explained very briefly. It frequently happens that uninterpreted mathematical calculi are used to perform the deductions which are required by a theory. This is clearly explained by Braithwaite (op. cit., Chaps. II and III). Where uninterpreted mathematical expressions are used, as for example, $x'' + n^2x = 0$, in order to facilitate deductive arguments by calling upon the apparatus of pure mathematics, it is necessary that somewhere a connection should be made between the signs appearing in these expressions and the scientific language. Here the process of translation is properly said to be carried out according to a dictionary. But the point which has been confused by exponents of the dictionary theory is that the *interpretation need not be into phenomenal statements*. The above equation is adequately interpreted for use in part of atomic physics by the following entries in the dictionary:

x is the amplitude and $2\pi/n$ the wave-length of stationary waves representing a single electron moving in a finite straight line in a field of constant potential.

It goes without saying that the concepts in this definition are not phenomenal.

4

We have now reached the point at which most methodological discussions *by physicists* begin, and indeed if they are not familiar with current philosophical writings on scientific method, they tend to regard such remarks as those above as trivial and obvious. But we are now in a better position to understand a confusion perpetuated by some writers in physics about the notion of 'unobservable entities'. What has been shown so far is that two different classifications of scientific statements have been confused by the dictionary theory:

(i) The distinction between theoretical and phenomenal statements, which does not call for *translation* but for *interpretation* of observations

at different interpretive levels involving more or less reference to theoretical concepts.

(ii) The distinction between formal and interpreted statements, in connection with which it is appropriate to speak of translation by means of a dictionary, but where the interpretation may be into theoretical rather than phenomenal terms.

To these two classifications must now be added a third, which corresponds to the use by physicists of the words 'observable', 'factual', 'objective', 'physically significant', as opposed to such phrases as 'mental conceptions', 'fictitious entities', and 'mathematical equipment'. In the philosophy of science since Newton the distinction implied by the use of such words as these has been invoked on various occasions and the pattern of thought is usually the same. Some piece of physics has raised difficulties either by contradicting common-sense conceptions of the world (for example Newton's action-at-a-distance) or by appearing self-contradictory (incompatible properties of the material aether or of fundamental particles). Philosophers or physicists of an empiricist persuasion have then asserted that the difficulty can be eliminated by regarding scientific theories which go beyond immediate experience as mere mathematical constructions, tools for correlating and predicting the results of possible experiments, but not as descriptions of physical reality. It is more comfortable to regard ourselves as using different tools for different purposes than to believe that we have found self-contradictions in reality. This was the line taken by Berkeley and Kant in relation to Newtonian science, and by Mach, Pearson, and Duhem in relation to nineteenth-century physics; it was generally, but not necessarily, associated with a phenomenalist theory of perception, and it usually blurred the distinction between the theoretical-phenomenal and formal-interpreted classifications of scientific statements. Before the twentieth century however, such epistemological discussion did not appear in physics itself, but only in commentaries upon it. The apparent contradictions within physics were resolved by learning to accept different kinds of model — central forces acting at a distance instead of Cartesian mechanism, or Maxwell's equations instead of a mechanical aether. But in twentieth-century physics, the word 'observable' has become a technical term of quantum theory, and physicists engage in apparent epistemological discussion *while doing physics*, so that the misleading impression is given that physics itself has come down on one side of the epistemological fence, and that this technical discussion is the same

THEORIES, DICTIONARIES, AND OBSERVATION

as that carried on by traditional phenomenologists and empiricists. Support is given to this impression when philosophers of science are found equating 'unobservable entities' with 'theoretical (as opposed to phenomenal) concepts'. We have already seen that it is misleading to speak of all theoretical concepts as unobservable, because all observation for the purpose of testing a theory involves some interpretation, and the interpretive report of an experiment may be given in theoretical terms. It is proper (and usual), for example, to say that the scattering of electrons by gas particles is *observed*, even though the phenomenal description of the observation would mention only white streaks in a cloud-chamber.

What then is the distinction which quantum theorists intend to make when they introduce the word 'observable'? I want to try to show what it is and that it has no bearing whatever on the epistemological problem already mentioned, although it may have important consequences for the philosophical problem of causality, but this cannot be discussed here.

Some quotations from an article by Heisenberg¹ will introduce some epistemological concepts in the context of an examination of the development of quantum theory:

The single quantum jump of Bohr, Kramers and Slater is 'factual' in nature; it 'happens' in the same manner as an event in everyday life, or the deflection of a galvanometer (p. 13).

All the opponents of the Copenhagen interpretation do agree [that] . . . it would . . . be desirable to return to the reality concept of classical physics . . . that is, to the idea of an objective real world, whose smallest parts exist objectively in the same way as stones and trees, independently of whether or not we observe them (p. 17).

Whereas positivism is based on the sensual perceptions of the observer as the elements of reality, the Copenhagen interpretation regards things and processes which are describable in terms of classical concepts, i.e. the actual, as the foundation of any physical interpretation (p. 22).

The 'actual' plays the same decisive part in quantum theory as it does in classical physics. . . . If we attempt to penetrate behind this reality into the details of atomic events, the contours of this 'objectively real' world dissolve—not in the mist of a new and yet unclear idea of reality, but in the transparent clarity of a mathematics whose laws govern the possible and not the actual (p. 28).

¹ "The Development of the Interpretation of the Quantum Theory." In *Niels Bohr and the Development of Physics*, ed. Pauli, London, 1955, p. 12.

And from Reichenbach:¹

Using the word 'observable' in the strict epistemological sense, we must say that none of the quantum mechanical occurrences is observable; they are all inferred from macrocosmic data. . . . There is, however, a class of occurrences which are so easily inferable from macrocosmic data that they may be considered as observable in a wider sense. We mean all those occurrences which consist in coincidences, such as coincidences between electrons, or electrons and protons, etc. We shall call occurrences of this kind *phenomena*. The phenomena are connected with macrocosmic occurrences by rather short causal chains; we therefore say that they can be 'directly' verified by such devices as the Geiger counter, a photographic film, a Wilson cloud chamber, etc.

Several points emerge from these quotations:

(1) It is clear that the 'fact' words; 'observable', 'actual', 'real', 'objective', are being used to refer to some events which are interpreted in the language of quantum mechanics and are therefore not reported in theory-free statements, and that the epistemological status of these events is regarded as equivalent to that of deflections of a galvanometer, stones, trees, etc.

(2) The theoretically interpreted events of which this is true are, generally, those which can be connected to phenomena (in the sense of this paper, not of Reichenbach's definition) by inferences using exclusively classical physics (a charged particle causes a scintillation on a screen whether in classical or quantum physics, and therefore the observation of a scintillation can be said to be an observation of the objective coincidence of a charged particle with the screen, whatever else may be said about this charged particle in any particular case).

(3) The distinction between objectivity and mental construction (actuality and potentiality in Heisenberg's language) is *not* the same as in positivism, where only sense perceptions are 'objective'.

(4) The 'details of atomic events' do not cease to be 'objective'; they merely cease to be describable *in the same way* as 'stones and trees, independently of whether or not we observe them'.

What emerges from this about the status of statements involving concepts of classical physics is quite consistent with what has been said

¹ *Philosophic Foundations of Quantum Mechanics*, Berkeley and Los Angeles, 1944, p. 20

THEORIES, DICTIONARIES, AND OBSERVATION

above on the relation between theoretical and phenomenal statements, and brings out the point that difficulties about 'unobservable entities' in the technical sense are peculiar to quantum physics. What these difficulties are, and how they differ from the traditional phenomenalist difficulty will now be considered.

Mach and Pearson regarded such entities as 'atom' and 'aether' as unobservable because they were not direct implications of sense-impressions; because they could not be seen, felt, tasted, smelled, or heard. But in ordinary language this is not sufficient reason for refusing to use the word 'observe' or language referring to the senses. We might speak of seeing aircraft when the only phenomena present are vapour-trails, and of speaking to X when we hear only a voice on the telephone. Perhaps we should not do this if we had never seen aircraft on the airfield or X sitting in our drawing-room. But this is only to say that mention of aircraft or of X implies that there will be other manifestations of them in given circumstances; that is they are not just *ad hoc* constructions out of vapour-trails and voices. And in classical physics there are many well-defined ways in which atoms and aether manifest themselves, all consistent with the assumption that they have the characteristics between observations which ordinary physical objects are assumed to have, namely that they behave according to the relevant laws of nature irrespective of whether they are observed or not. The fact that they manifest themselves in different ways consistently with these laws of nature means that they are not *ad hoc* constructions, and the fact that we never meet an atom face to face or go for a swim in the aether does not mean that we have no right to regard them as objective entities. It is just that they are not the sort of entity that one does meet face to face or swim in. The laws of nature which are relevant to them are not necessarily identical with those of physical objects, although, as with physical objects, these laws can be regarded as consistent descriptions of the atomic entities when these are unobserved.

Such is the epistemology which Heisenberg and Reichenbach, in contrast with Mach and Pearson, wish to employ when speaking of the entities of classical physics, and of modern physics in so far as these behave classically. The epistemological difficulty here is one which is common to classical and modern physics and turns on the propriety of using the word 'observe' when reporting the results of experiments in theory-loaded terms. We have already seen that experiments have to be so reported, but it may still be objected that the theoretical nature

of these reports should be safeguarded by avoiding the suggestion that atomic entities etc. are being directly observed.

I am inclined to think this objection ill-founded, and will try to show why by examining some remarks by Professor Körner.¹ After defining 'ostensive rules' as those which 'all or almost all human beings (after a certain stage in their development) accept' (p. 7), he speaks of passing from 'signs the use of which is governed by ostensive rules to other discursively used signs such as . . . "electron". . . the latter cannot in any straight-forward manner be regarded as applicable to anything' (p. 17). But he also thinks that 'many such scientific concepts . . . are only provisionally non-ostensive. . . . An atomic nucleus might conceivably some day be pointed out' (p. 52), and 'It is in principle possible to observe atoms directly' (p. 60), although later he briefly suggests that talk of 'what is in itself unobservable' being indirectly observed through its effects, or inferred from what is observed, is unclear and would be better expressed differently (p. 193).

If only ostensive rules which are universally accepted are to count, then indeed atomic entities are non-ostensive, but it is not clear how in this case they could ever be pointed out as Körner suggests. An atomic nucleus is not the kind of thing one could unmistakably meet in the street, and if what is meant is that it could be pointed out in something like an electron micrograph, then it is still only the physicist who can interpret the marks there seen as an atomic nucleus, and this interpretation is no different in principle from the interpretation of cloud-chamber tracks. It might of course be the case that human beings universally learnt the physicist's language and shared his laboratory experiences, but if this is what is meant it seems that the distinction between ostensive and non-ostensive is mainly sociological and only logical in the sense of referring to different interpretive levels, as Körner himself explains in connection with ordinary physical objects (*ibid.*, p. 136 sqq.). It is admittedly easier to be *mistaken* about highly interpreted observation, but this does not show that it is not observation, for observation, as we have seen, is never incorrigible.

The distinction which is often implied between observing atomic particles 'directly' in micrographs, and observing them 'indirectly' by means of cloud-chambers or Geiger counters seems to be a misleading extension of the analogy with physical objects, and one which does not clarify the logical function of observation in relation to theories. The distinction is made plausible in the first place because in micro-

¹ *Conceptual Thinking*, Cambridge, 1955

THEORIES, DICTIONARIES, AND OBSERVATION

graphs, if we assume the analogy between atomic entities and ordinary particles, we can be said to be 'seeing' the atom in the same sense as we 'see' an object x in its photograph. The relation between x and its photograph expressed in terms of light or electron beams is the same as that postulated in the particle model between the atom and its photograph, even though in the case of the atom nothing corresponds to 'seeing' x in the straightforward sense. In the case of cloud-chamber tracks of electrons, however, we may be more reluctant to use the word 'see' because in the analogous case of aircraft making vapour trails, we might prefer to say that the aircraft was the *cause* of the trail, not that we saw the aircraft directly. But this is because we know what it is to see the aircraft directly. In the case of electrons, photons, and mesons we do not know what it is to see them directly in this sense, in fact such a 'direct seeing' is in principle impossible, at least in the case of photons, because they are the very media by which we see directly, and no mechanism of so seeing them can theoretically be conceived. Are we then forced to say that all observation of them is 'indirect', whereas observation of an atom in a micrograph is 'direct'? This does not seem to be necessary. Each entity is observed in the ways appropriate to it and not in ways which are in principle impossible, and the distinction suggested does not point to any important logical difference in the way the entities function in the theory.

This distinction must not be confused with another which is logically important: namely that concerning observations which are said to constitute the 'discovery' of a new particle. The neutrino was properly said to be 'discovered', when independent means were found of detecting the presence of a particle which had previously merely been postulated to account for lack of energy balance in certain nuclear reactions. Until these means were found the particle might have been merely an *ad hoc* construction invoked to save the conservation of energy, but to show that it was not that, it was not necessary to 'see it directly' in any kind of photograph, but simply to detect it in some way appropriate to the detection of sub-atomic particles, and the detection was in fact in terms of tracks. It is the independence of various observations which ensure the 'real' (non-*ad-hoc*) character of atomic entities, not any distinction between direct and indirect observation.

To return to the physicists. What they wish to contrast with the observable is not the *unobservable* but the *unobserved*. Their difficulty about the unobserved is not a general difficulty about theoretical

concepts as such, but a particular difficulty about the quantum theory. Reichenbach explains what it is by pointing out that in the usual cases of physical objects and the entities of classical physics it can be consistently assumed that the laws of nature relating to them are the same whether they are observed or not, and that if there is no external influence upon them other than the very small influence necessary for observation, their state remains the same. (The tree is always about in the quad.) If an unobserved system can be so described, Reichenbach calls it *normal*. It can however be shown to be *self-contradictory* to assume this in the case of the particles of quantum physics when they are unobserved, since the occurrence of an observable event in connection with them (coincidence with a photographic plate, etc.) changes their state in unpredictable ways. What is unobservable is not the particle as such, but its state when unobserved. This sounds like a tautology, but is not, because it may be said of a billiard-ball that its state is observable when unobserved in the sense that its position and momentum can be consistently inferred from their values when observed, and that another observation will confirm this inference. This is not the case with atomic particles. It is important to notice, however, that this conclusion follows only if the analogy between microscopic and macroscopic (mechanical) events is assumed, that is, if the particle-model for microscopic events is assumed. It is not *logically* impossible that a quite different theory of the microcosm might enable all relevant events and entities to be described in terms of what Reichenbach calls normal systems. But these events and entities could not be particulate in the classical sense.

I conclude that the use of the term 'observable' and its cognates by physicists in quantum-mechanical contexts has no special bearing on epistemological controversies. What is epistemologically interesting about quantum theory is that it provides a clear example of the inadequacy of the dictionary theory, and has to be described in terms of a hierarchy of interpretive levels, each introducing a language richer than that of the preceding level. It has been the object of this paper to examine the nature of such a description.

Dept. of the History and Philosophy of Science
University College
London

THEORY-CONSTRUCTION AND THEORY-TESTING ★

PETER ALEXANDER

I wish to defend 'the dictionary view' of scientific theories against Miss Hesse's attack and to argue that she has insufficiently considered her assertion that phenomenal statements do not 'constitute a basic empirical language which is independent of all theory'. I agree that the word 'dictionary' is likely to mislead in this context and that the 'theory-loading' view of phenomenal statements, much in vogue among Angry Young Philosophers, contains a grain of truth. But, on the whole, I am a reactionary in these matters.

So reactionary am I that I would be prepared to talk of observation statements, but I defer to Miss Hesse in this. However, I should like to introduce another term. Miss Hesse means by 'phenomenal statements' ('almost always') those which 'do not describe what happened on a particular occasion to a particular observer, but what always happens and will happen on sufficiently similar occasions to all normal observers' (p. 15). Since the testing of theories is done by individuals who can only be in one place at a given time, I wish also to refer to 'test statements'. These are whatever singular statements are used, in the last resort, to test a theory, are directly verifiable and, at their simplest, are those singular statements which can be deduced from Miss Hesse's general phenomenal statements together with singular existential statements. Depending on the context, test statements may contain technical terms of some scientific theory or they may be in the simplest common-sense language understandable by all normal people. In happier circumstances I should call these last, at least, 'observation statements'.

Miss Hesse is, it seems to me, playing the part of Prospero; with her permission I should like to cast myself for the part of Caliban. The root of our disagreement lies in our taking views of scientific theories appropriate to these characters. Prospero sees science as a body of

★ Paper read in reply to Parts 1, 2, and 3 of Dr M. B. Hesse's article at the second annual conference of the Philosophy of Science Group, Nottingham, 21st September 1957.

theory to be tested and asks, primarily, 'Does my magic work'? Caliban sees, first, a collection of puzzling phenomena which he would like explained and asks 'How can this be accounted for?' Prospero knows 'how to bedim the noontide sun and call forth the mutinous winds': Caliban knows where to find fresh springs, pig-nuts, and marmosets. Prospero puts his faith in his theories and grudgingly admits phenomenal statements because of their superior certainty: Caliban takes phenomenal statements as basic because they are what he wants explained. This is a difference of emphasis which has far-reaching effects. If we regard a phenomenal statement as primarily something for testing theories we are likely to reach conclusions about it different from those we reach if we regard it as something which needs explaining. But phenomenal statements must be accepted, in the first place, not because of their superior certainty but because they are what the scientist sets out to explain.

That Miss Hesse is correctly cast as Prospero emerges throughout her paper. She is almost entirely concerned with the testing of theories and hardly mentions their explanatory function. Because she works always from the theory downwards to the phenomenal statements she sees phenomenal statements as always involving the terms of the theory.

I do not wish to argue that Miss Hesse's view is entirely unjustified. The scientist is neither Prospero nor Caliban; he is Prospero-Caliban. I wish merely to plead for Caliban. The scientist is a dreamer of dreams who finely spins theories but we must never forget that he is also a grubby, worried little man who stains his fingers and drops weights on his toes. If we ignore either of these things we get, at the best, a one-sided picture of science and this, I suggest, is what Miss Hesse has done. There are two sorts of operations in which the scientist engages; first, that of noting puzzling phenomena and searching for explanations of them and, second, that of looking for, or trying to produce, other phenomena which will support these explanations. I shall argue that the descriptions of phenomena involved in the first pursuit logically cannot, in one sense, be theory-loaded and that if those involved in the second pursuit are often, in practice, theory-loaded they are, and must be, capable of being unloaded.

Science begins when we begin to search systematically for explanations of phenomena. The original question is 'How does that happen?', which any man, call him A, may ask about something he sees. His companion, let him be B, may ask 'What are you talking

THEORY-CONSTRUCTION AND THEORY-TESTING

about?' and then A has to describe what he sees. 'This stick is straight but it looks bent when partly immersed in water.' And B, if he is neither blind, nor illiterate, nor an idiot, will say 'Oh yes, I *see*, how odd'. This, incidentally, is the point at which all the fuss about basic propositions or protocol statements is relevant. It has nothing to do with whether a description of an experiment can be given without using scientific technical terms. It is concerned with whether or not we can be certain of agreeing about the phenomenon before ever an explanation is sought for or proposed. As I do not think this has the slightest relevance to our main theme, I shall not dwell on it.

But to return to A and B. So far they are Calibans, puzzled by what they have seen. Now they may begin to try various explanations, and the question they ask about each is 'Does this explain it?' *not* 'Does what we see support, or test, our theory?', since there is no point in trying to *support* the theory unless it can do the primary, explanatory job.

Now, it may be argued that in every example even the original description of the phenomenon is theory-loaded, in the sense that the terms 'straight' and 'bent' depend upon some primitive geometrical theory *or* that the peculiarity of the phenomenon is only understood if it is already accepted as a law that sticks that look straight normally go on looking straight. The first point has no substance since 'straight' and 'bent' can surely be regarded as names for certain appearances, unconnected with any theory. If the second point indicates theory-loading it is of a sort which is unobjectionable. All we need demand is that the original description of what is to be explained must not be loaded *with the theory which is intended to explain it*. This is my first main point.

Our description of a given phenomenon may be altered when we accept a theory as explaining it but the new description cannot function as a test statement for *that* theory. It may, of course, function as a test statement for a new and wider theory but, again, it must not be stated in the terms of that new theory. Depending on the theory in question a test statement must be a description which either is not theory-loaded or is loaded only with accepted theories which are not under test. Such theory-loading is not fatal to science as an explanatory system as, I submit, Miss Hesse's view is.

I say it is fatal on logical grounds. We must remember that to say that a statement confirms a theory is to say that it is explained by that theory. In general, if *e* is to explain *p* then *e* and *p* cannot be identical

nor can either contain the other except in the sense in which the premisses of a deductive argument contain the conclusion, otherwise we have not got an explanation. The explanation of something must be different from that something. Applying this to scientific theories we can say that technical terms invented to explain our description of phenomena must not be included in that description. That is, the technical terms of a theory which have been introduced *just for the purpose* of explaining *p* must not themselves appear in *p*, although other technical terms of already accepted theories *may* appear in *p*. If this condition is not fulfilled the theory will not explain *p* and *p* will not serve as a test of that theory.

Consider a trivial example adapted from Campbell.

p: 'When milk freezes it pushes the top off the bottle.'

e: 'That is because milk, unlike other liquids, expands over certain temperature changes.'

This is a primitive explanation but it would not explain *p*: 'When milk freezes it *expands* and pushes the top off.' An explanation of this would require a statement which did not depend upon expansion but *explained* this expansion. On these grounds I conclude that if Miss Hesse is correct in holding that any description of phenomena which is to test a theory must be loaded with that theory, then a scientific theory can never function as an explanation. I am not sure that she wishes to make this last assertion.

When Miss Hesse says that 'the claim that phenomenal statements are unambiguous cannot be maintained' she is, I think, confusing doubts about whether phenomenal statements can be certain with doubts about whether they depend upon any scientific theory. That is, she is irrelevantly bringing in arguments about protocol statements. I am not sure who, nowadays, would say that phenomenal statements are unambiguous but it is certainly not implied, as she says it is, by 'the assertion that the meaning of the statements is independent of any theories' (p. 15). There are other ways of being ambiguous than by being theory-loaded. But, more seriously, I am not sure that anyone has asserted 'that the meaning of the statements is independent of any theories' and no adherent of the dictionary view *need* make it. Campbell says 'it must be possible to determine, apart from all knowledge of the theory, whether certain propositions . . . are true or false.'¹ This means, I suggest, that the test statements for a given theory

¹ *Physics: The Elements*, Cambridge, 1920, p. 122. Quoted by Miss Hesse, p. 12 (my italics)

THEORY-CONSTRUCTION AND THEORY-TESTING

must not be loaded with *that* theory, which is what I have been asserting. I do not think that the other four versions of the view make the mistake Miss Hesse accuses them of but, even if they do, it is not inherent in the view that they must.

I am not convinced that Miss Hesse has been wise in grouping these five statements of the view together and discussing them as if they were one. I have time only to take as an example the versions of Campbell and Ramsey, and to insist that Ramsey did not follow Campbell as closely as she seems to think. Their 'dictionaries' serve quite different purposes. While Campbell might,¹ I think, accept with reservations that 'the meaning of theoretical statements is dependent upon a dictionary which translates some of them into phenomenal statements', Ramsey gives *his* dictionary² the different function of connecting two parts of an abstract system neither of which, at this stage, contains phenomenal statements. His dictionary gives definitions of formulae of one part of the system in terms of formulae of the other part. Either part may then be *interpreted* in phenomenal terms but this is not done with the help of the dictionary. For Ramsey, defining and interpreting are different processes. He perhaps obscures this to some extent by giving, for pedagogic reasons, an example of a possible interpretation of the system in the course of constructing it. But he is concerned, like Braithwaite, to build up a *calculus* which shall show the logical structure of a scientific theory. When this calculus is interpreted in terms of experiences it will be shown to apply to existing scientific theories. But the dictionary belongs to the calculus and is not a tool for interpretation.

Braithwaite and Woodger are, I think, closer to Ramsey, Ayer closer to Campbell in this, though I doubt if any of these authors would accept either Campbell's odd view of meaning or his anti-logical assertions of Chapter II. I shall not, however, press my demand for separate treatment of these versions since Miss Hesse's main criticism of the dictionary view, as she conceives it, is, I wish to argue, without foundation and does not render any of these versions untenable.

The two alleged features of this view upon which she bases her criticism are:

'that the meaning and criteria of truth of phenomenal statements is independent of that of theoretical statements' and 'that the meaning

¹ Op. cit., p. 122-123

² *The Foundations of Mathematics*, London, 1931, pp. 214 sqq.

of theoretical statements is dependent upon a dictionary which translates some of them into phenomenal statements' (p. 14).

Leaving aside the difficulties about the uses of the dictionary, the second alleged feature needs much refining to make it fit, for it is possible to say that theoretical statements have *some* meaning (mathematical, analogical, etc.) apart from those phenomenal statements they go towards explaining. But this need not delay us since Miss Hesse deals mainly with the first of these features which is a claim of all five versions of the view.

The criticism which Miss Hesse regards as fatal does not touch this view or any of its implications. She argues that 'if phenomenal statements are to be tests of a theory they cannot be independent of the theory' (p. 16). If phenomenal statements are to have scientific significance 'there must be connections of meaning between them at a higher than common-sense level, and therefore the condition of complete theoretical independence between them must be dropped' (p. 16). 'Physics can be reduced to phenomenal statements if these connections of meaning are ignored, but such reduction does not give a full account of the observational basis of physics, for it cannot explain why just these statements are chosen and not others' (p. 16).

Now, in the first place, if Miss Hesse is arguing against reductionist accounts of scientific theories I am with her, and so, I should have thought, were Campbell, Ramsey, Braithwaite, Ayer and Woodger. This is surely an important feature of the dictionary view. The assertion that phenomenal statements are deducible from sets of theoretical statements and existential statements does not imply that these theoretical statements are translatable without remainder into phenomenal statements. Campbell claims that theoretical statements are significant apart from the dictionary and only some of them translatable.¹ Ramsey was precisely concerned to dispute the reductionism implicit in the logical constructions view. Ayer rules out reductionism in his explanation of 'directly verifiable'.² Professors Braithwaite and Woodger can no doubt speak for themselves on this.³

But, in the second place, and more importantly, surely none of these authors denies that there are connections of meaning between phenomenal statements at a higher than common-sense level, nor is such a denial implicit in their accounts. Such connections are precisely what

¹ Op. cit., p. 122

² *Language, Truth and Logic*, London, 1950, p. 13

³ But see Braithwaite: *Scientific Explanation*, Cambridge 1955, pp. 50-53

THEORY-CONSTRUCTION AND THEORY-TESTING

scientific investigation tries to establish and this is one of the features to which the dictionary view tries to do justice. In his discussion of the Kinetic Theory, Campbell brings out the way in which the Theory shows that connections between perfectly elastic particles underlie, for instance, connections between the pressure, volume, and temperature of a gas. This is allowed for in his account of theories. The higher than common-sense connections are to be found among his 'hypotheses'. These must be significant *apart from the dictionary* but, apart from the dictionary, appear to be arbitrary assumptions. By relating *some* of the hypothetical propositions to *some* phenomenal statements the dictionary removes this arbitrariness. The connections between the hypothetical propositions, some of which imply and some of which do not imply phenomenal statements but all of which are significant, *are* the higher than common-sense connections between those phenomenal statements. Much the same sort of thing can be said about the other versions.

Of course Miss Hesse is right when she says that if phenomenal statements are to be tests of a theory they cannot be independent of the theory, but whoever thought they could? What is asserted is something quite different, namely, that if a phenomenal statement is to be a test of a theory it must be understandable without reference to *that* theory. Showing that a statement which we understand is derivable from other statements shows us more about its connections with these others but it does not lead us to understand anything more or different by *that* statement. The meaning of 'Socrates is mortal' is not changed when we discover that it can be deduced from 'All men are mortal and Socrates is a man'. There must, of course, be connections of meaning between phenomenal statements and the theory that explains them otherwise the theory would not explain them, but this does not imply that the meaning of a statement is altered when a theory is found to explain it or that it cannot be understood without knowledge of that theory. That this is so is abundantly evident in the history of science, for many connections on the phenomenal level have been known before any theory was invented to explain them. Boyle understood by the statement that $p v = \text{constant}$ just what Clausius and Maxwell understood by it even though Clausius and Maxwell knew more about the possible *explanations* of it.

Miss Hesse is in her most Prospero-like mood when she enlarges on her view that 'a phenomenal statement can never test a theory alone, but only a theory together with its dictionary interpretation into this

particular phenomenal statement' (p. 16). This, and much else, suggests that scientists sit in their studies spinning theories out of thin air and then set about looking for phenomenal statements to test them. In consequence, she is worried about how we know that a given interpretation of a theory into phenomenal statements is relevant to the theory. But surely the way in which we start from puzzlement about phenomena ensures that our theory will not be wildly irrelevant. Consider Miss Hesse's example of Snell's Law. We know how to interpret $\sin \alpha / \sin \beta = \mu$ because we arrive at this constant relation by examining the phenomenon, drawing diagrams and measuring what appear to be relevant angles. In a sense, we *begin* with the interpretation and work upwards to the formalism. We choose *this* formalism because we have arrived at it through the phenomenon. So it is, I suggest, with most theories, however much more complicated they are than this. (See Braithwaite's zip-fastener analogy.) Miss Hesse puts the cart before the horse. The possible interpretations of α and β are not, at this stage, 'already circumscribed by the theory' or, at least, this is a misleading way of putting it. The theory is circumscribed by the context of the observations which lead to it.

When we come to test the theory by other phenomena, such as the burning of paper by a lens, we, of course, test our interpretation in a new field and here the interpretation *is* circumscribed by the theory. But what we test is whether the phenomenal description follows from an account of the bending of light rays. It is the relation between the description in terms of the theory and the ordinary phenomenal description, using none of the terms of the theory, which is important. Approximate judgments of relevance which guide any variation in interpretation for the new phenomenon are, as Miss Hesse says, common-sense judgments: the testing of the theory is itself the determining of the precise relevance.

Miss Hesse's further example concerning scintillations and the neutrino (p. 18) can be similarly dealt with, although it appears more favourable to her view simply because it concerns a 'higher level' in the theory. Relative to the theory concerning the presence of an undetected particle, the description of the scintillations together with the account of decay processes in the reactor is a basic or test statement. That is, the observations plus the established theory are used to test an *addition* to that theory, but what is directly observed (scintillations) could be described without reference to any part of the theory, and *this* description would be a test statement for the *whole* of the theory.

THEORY-CONSTRUCTION AND THEORY-TESTING

In her remarks about the teaching of the neutrino theory, Miss Hesse has, I think, failed to distinguish between understanding a test statement, which does not require knowledge of the theory, and understanding that it *is* a test statement for that theory, which does require such knowledge.

I find her conclusions about the Wilson cloud experiment exceedingly puzzling. The phenomenal description of apparatus, lines drawn and angles measured, she says (p. 20), 'clearly . . . means nothing and could test nothing, because it does not state its relevance to anything'. But in the context in which it is likely to be thought to test a theory, there would already be reason for thinking it relevant to the theory. The test would determine its precise relevance by showing it possible (or impossible) to deduce from the higher level statements that if the observer, under certain conditions specified in phenomenal statements, drew certain lines and measured certain angles he should get the observed values. The theory has, after all, been constructed from *other* basic phenomenal statements which have led to its mention of photo-electrons and so on. The context of the new observations suggests its application to this phenomenon and what we test is the relevance of this suggestion. But the fact that just above the new phenomenal statements in our deduction we have non-phenomenal statements does not make the phenomenal statements any the less phenomenal. We can, as it were, 'shed' the theoretical terms as our deduction proceeds until we finally reach statements about the results we should get if we draw lines and measure angles. There is no need for a phenomenal statement to proclaim its own relevance to the theory. The theory proclaims its relevance to the phenomenal statement if it fits.

Campbell's primitive theory about the resistance and temperature of metals brings this out.

$$(c^2 + d^2)a \left| \frac{cd}{b} = zab = \text{const. is deduced from his hypothesis.} \right.$$

By means of the dictionary it is interpreted as 'the ratio of the resistance of a piece of pure metal to the absolute temperature is constant'. Given that the meaning of all these terms are accepted by us we need no further translation but with the help of these accepted meanings we can translate it further, for the layman, into purely phenomenal terms, i.e. terms which do not require knowledge of the scientific theories of electricity, temperature, or chemical purity. This is all deductive, and

PETER ALEXANDER

the fact that our deduction takes us through statements which cannot be understood without scientific knowledge does not imply that the lowest level statements cannot be so understood. Moreover, the fact that Campbell's theoretical statement can be translated into phenomenal statements does not imply that all his hypothetical statements can be so translated, for some of them may function purely as steps in the deduction near the top of the chain and not 'come out' at the bottom. All this is brought out also by the systems of Ramsey and Braithwaite.

There are more ways than one of giving a commentary on what scientists do. Miss Hesse has, I think, a tendency to regard her five authors as doing some kind of psychologico-historical study of science, *to see them thinking as scientists think*, and this has led her to misconceive their results. They are claiming *not* to show how scientists work but to present a 'rational reconstruction' of their finished theories. They are not claiming that scientists 'set up a theoretical deductive scheme' (p. 17) and then search for possible interpretations of it but rather that such theoretical deductive schemes can serve as models which exhibit the logical relations between the statements from which they start and those with which they conclude. Campbell and his descendents emulate neither Prospero nor Caliban, but Shakespeare.

Dept. of Philosophy
University of Bristol

NOTE AND COMMENT

On Multi-dimensional Time

MULTI-DIMENSIONAL time variables have been discussed in this *Journal*¹ in connection with highly controvertible psychological speculations. Here I just wish to point out that two-dimensional time and, more particularly, complex time need not be introduced *ex hypothesi* in physics, being already included in theories of spinning particles.

In fact, it has been shown² that six new constants of the motion of the free electron are obtained by performing a certain transformation involving the new co-ordinates

$$X^\mu = x^\mu + (A/2)i\gamma^\mu, A = h/m_0c, \mu = 0, 1, 2, 3,$$

where $i = \sqrt{-1}$, ' h ' designates Planck's constant divided by 2π , ' c ' represents the velocity of light in vacuum, ' m_0 ' stands for the electron rest mass, and ' γ^μ ' denote Dirac's 4×4 matrices. The eigenvalues of the space part, \mathbf{X} , of this operator are real and may be interpreted as the centre of mass co-ordinates of the electron, the mass of which (but not the charge) can be pictured as spread over a sphere of average radius $\sigma = A/2$. (The factor $1/2$ drops out in the case of spin 1 particles.) On the other hand, the eigenvalues of the time component of that operator are complex:

$$\text{cit } X^0 = ct \pm i(A/2)$$

The constant time $\tau = A/2c$, the value of which is of the order of 10^{-21} sec. in the case of the electron, may be interpreted as the spinning period of the electron charge.

As can be seen, there is no mystery in the above complex time operator X^0 ; it just represents a pair of real times, to wit, the extrinsic time variable t , and the constant intrinsic time τ , related to the internal motion of the particle. Complex quantities entering physical theories are—provided they do not play a barely intermediary syntactical rôle—just pairs of real quantities having each a physical correlate. Nothing unreal and nothing psychological is implied by their use.

MARIO BUNGE

¹ H. A. C. Dobbs, 'The Time of Psychology and of Physics', this *Journal*, 1953, 4, 162. C. T. K. Chari, 'A Note on Multi-Dimensional Time', this *Journal*, 1957, 8, 155

² M. Bunge, 'A Picture of the Electron', *Nuovo Cimento*, 1955, ser. X, vol. 1, p. 977. Preliminary results on similar work concerning spin 1 particles were reported by the author in 'Sobre la imagen física de las partículas de spin entero', *Rev Unión Matemát, Arg. y Asoc. Fís Arg.* 1957, 18, 74.

DISCUSSION

ON THE SPACE AND TIME OF IMAGES

THE purpose of this note is twofold: (1) to reply to certain points raised by C. T. K. Chari in a recent note in this *Journal*¹ and (2) to make certain modifications to my theory suggested by Professor Broad.

1. Chari states that we have no criteria for judging the geometry of hallucinatory spaces and that we have no measurements or conventions to determine the 'metrisability' and dimensionality of the space of mescal visions. In which case we should not talk about an image space as though it could be conjoined in some way with physical space, for which we do have such conventions and means of measurement. To approach this let us distinguish a hierarchy of questions: (a) 'Are visual images (I include here visual hallucinations, mescaline-induced images, hypnagogic and eidetic images) in some sort of space?' This may be put 'Do visual images have (b) topological and/or (c) metrical properties?' I would argue that images *do* have the topological properties that they appear to have on simple inspection. For instance an image has a spatial *boundary* that uniquely divides the field in which it is embedded into one *inside* and one *outside*. Professor Price² has emphasised that one image can bear various spatial relations such as 'to the left of', 'above', 'below', etc., to one or more other images and Nicod³ has minutely specified the spatial relations of sense-data. Two hallucinated squares cannot intersect more than twice any more than can two squares drawn on paper, whereas an hallucinated square and rectangle can intersect up to four times. We should say that images and hallucinations *satisfy* some simple axioms and theorems of topology. It would be interesting to observe how many of the more complex theorems are also satisfied by images.

The next question is 'Is such a space metric?' Again the answer seems to be 'yes', for the metric of physical space is *based* ultimately on such perceptual observations as 'a is longer than b', or that a certain mark on a scale is opposite an end of something, or that the tip of a pointer is opposite a mark on a dial. Physics reflects the complex numerical relations obtainable in this way. Now, if we examine any field of hallucinatory images, we will also be able to discover that the same relations hold for this field, i.e. that one image is much smaller and shorter than another and that one mark is opposite another. The numerical relations between the lengths and relations of such images may only be confused, but this only means that we are not able to build a physics out of our observations; it does not mean that the entities observed do not possess a geometrical order themselves. We cannot take any

¹ C. T. K. Chari, 'On the "Space" and "Time" of Hallucinations', this *Journal*, 1958, 8, 302-306

² H. H. Price, 'Survival and the idea of "another world"'; *Proceedings of the Society for Psychical Research*, 1953, 50, 1-25

³ Jean Nicod, *Foundations of Geometry and Induction*, London, 1930

ON THE SPACE AND TIME OF IMAGES

particular sense-datum or image as a unit of image length (as we can take a convenient stick) because the definition of any particular image or sense-datum is valid only for the instant at which it is made. Sense-data are shifting parts of a more permanent whole—the visual field—as images are part of what we can call the image field—and the only metrical judgment that can enable us to compare sense-data not compresent is the relation to the whole visual field, i.e. the amount of the total field that they cover. Chari objects that we cannot metrise image space because of the unstable character of sense-data. But this objection merely shows that we cannot use one sense-datum as a permanent rule to measure other sense-data, and that Chari has not analysed the concept of metrisability at a sufficiently fundamental level. We can accept here Dingle's definition¹ of measurement as any precisely specified operation that yields a number. Clearly such operations can be carried out on images and hallucinatory sense-data as I have described above and in more detail elsewhere.²

So we can establish, on the basis of a mere inspection of images, that they do have some topological and geometrical properties, i.e. that image space has *some* kind of geometry. But it may not be legitimate to ask if its geometry is Euclidean or non-Euclidean. These concepts may express levels of relationships at a level higher than we can go with the primitive contiguities and congruences of images. The sum of the three angles of an imaged triangle add up to something like 180° but we cannot be any more specific than that. We can be certain, however, that at times we can observe sensory images that have three angles, others with four, five, none, etc. We can also say that image space is itself three-dimensional, for we need three and only three co-ordinates to specify the position of any hallucinatory sense-datum in an hallucinatory sense-field. In any case for the purpose of relating image space and physical space it is only necessary to establish that image space is a topological and metric space of some kind. The details of the spatial relations between images need not affect the gross specifications of the relations between images and objects.

If we then agree, after a close inspection of, say, a clear little hypnagogic image, that it does have both topological and metrical properties—that Klüver's spiral form constant is a spiral—we can then ask our next questions, 'Does this image bear (*d*) topological and/or (*e*) metrical relations to physical objects?' Professor Price³ has recently put forward an affirmative answer to question *d*. He postulated that images, although in a space of their own, bear no spatial (metrical) relations to objects but only causal relations. The topological relation of 'external to' is implied in this hypothesis. Lastly a 'yes' answer to question *e* may be amplified by noting the *possible* geometrical relations between one 3-spaced and another and by asking how many image spaces there may be, i.e. do we all share one or do we each need one to ourselves? Previously I had assumed that one needed a $(3m + 3)$ —space to accommodate *m* human individuals and their individual image spaces but this was an error as appears below.

2. Professor Broad, who was the first to suggest⁴ that the space of sense (and images) and physical space were some kind of section of a space-like whole of more than three dimensions, has pointed out that to accommodate physical space and *m* image spaces we

¹ H. Dingle, 'A Theory of Measurement', this *Journal*, 1950, I, 5-26

² J. R. Smythies, *Analysis of Perception*, London, 1956

³ H. H. Price, loc. cit.

⁴ C. D. Broad, *Scientific Thought*, London, 1923

J. R. SMYTHIES

need only a 7-space of which physical space and image spaces are merely sections. The main requirement to be met is that no part of one space should be coincident with any part of another (for my images are not all mixed up with your images nor with physical objects). Thus we can imagine one common physical space and m private image spaces stacked 'around' it (as, in a lower dimensional analogy, cards are stacked in a pack or pages in a book), and all these sections are ordered in a 7-dimensional manifold. There may be other ways of relating $(m + 1)$ sets of 3-spaces so that they are not coincident at any point but this seems to be the simplest. Professor Broad has also very kindly made the following points in connection with my argument in *Analysis of Perception*:¹

(i) The problem of the non-sensible part of sensum space (which is taken to be identical to image space) may be introduced by noting that from a consideration of the geometry of the visual field there could be unsensed sense-data, or causal ancestors of sense-data not themselves sense-data, outside the visual field and yet in the same 3-space as the visual field. (Hence what I have rather crudely called signalling mechanisms interposed between the brain and the visual field can more elegantly be thought of as an integrated series of causal ancestors of sense-data not themselves sense-data, outside the visual field and yet in the same 3-space as the visual field (and hence *not* in the same 3-space as physical objects) and whose function is to order sense-data in line with the relevant brain events. This gives us a model for possible intermediate steps in the causal relations postulated between brain events and the behaviour of sense-data in the visual field.)

(ii) The distinction between theories IA and IB and between IIA and IIB can be formulated better. The sensible fact that image and sensum space is limited may be expressed either by saying that it is a *part* of a larger space of the same dimensionality (as was expressed in (i) above), or by saying that it is finite but unbounded rather than by saying, as at present, that it is a small 'bubble' of space surrounded by 'no space'.

(iii) If there are only causal relations and no spatial relations between sense-data and images on the one hand, and physical objects on the other, the causal interaction between them is not 'non-spatial' but action at a topological but non-metrical 'distance'.

J. R. SMYTHIES

Galesburg State Research Hospital
Galesburg, Illinois, U.S.A.

¹ London, 1956

REVIEWS

PROBABILITY AND INDUCTION

It is an undeniable fact that scientists agree fairly well in what they do in the laboratory but disagree very much in explaining what they do. Experimental procedure is hardly ever in doubt, but scientific method is. Scientists might use vaguely similar terms for characterising their methods, though what they mean by them are obviously very different things. Thus they say that their methods are *inductive* and that these establish the results of their research with a certain *probability*. For most scientists are conservative and follow the philosophical tradition in which they were brought up when they speak about their work, however revolutionary they are in the work they are doing. They say that they 'infer from the facts' and that, since facts are stubbornly independent of human beings, the inference confers only a certain degree of probability on the conclusion. What is meant by 'induction', 'inference', and 'probability' in this context is not at all clear. What is most irritating is that they are all good practical statisticians and use the mathematical calculus of probability with skill and exactitude. More often than not, they even manage to reach the same solution to a particular problem. But if one asks for an explanation of the method by which the solution was obtained, hardly one statistician ever agrees with another. On the contrary, statisticians are bitterly critical of each other and are prone to convict their colleagues of the basest fallacies. It is not only the layman who thinks that 'with statistics you can prove anything'.

The main reason is that, however technical and formal probability theory has become, the basic conceptions are beclouded by ancient metaphysics. Probability and statistics are supposed to be the tools of a so-called inductive inference; and 'Inductive inference is the only process . . . by which essentially new knowledge comes into the world' as Professor Fisher has remarked in one of his earlier books. With this (or some similar) statement we are led right back to about 400 B.C. when the problem 'What is Knowledge?' was first posed in a systematic manner. Its solution determines what we take as scientific inference or 'induction'. This is the ancient game of epistemology: the pieces we choose—that is, the key concepts—determine the rules of the game and vice versa.

The three books¹ under review are examples of this traditional game.

¹ R. A. Fisher, *Statistical Methods and Scientific Inference*, Oliver and Boyd, Edinburgh and London, 1956, pp. xiii + 175. 16s.

REVIEWS

Though they differ widely in their 'pieces' and in the conclusions reached, they agree in the attitude of *inductivism*. Let me add immediately that all three books are of the highest possible calibre and contain a great deal more besides inductivism : but most of it is severely technical, and I must confine myself here to some main topic of interest to the philosopher of science.

Professor von Wright, for example, discusses the philosophical history of the problem of induction and, in particular, the views of Hume and Kant. More than half of Professor Jeffreys's book is concerned with the application of scientific method to diverse fields ranging from Newtonian dynamics to quantum mechanics. Professor Fisher presents various forms of 'quantitative' inference in mathematical detail.

Two of the books, those of Jeffreys and von Wright, are new editions of well-known treatises which have been, however, partly re-written ; while Fisher's book, though new, is a re-statement of ideas known to us from previous publications of the author. This is an additional reason for restricting the review to the main topic of probability and induction.

There are three main schools of thought regarding probability. One school identifies probability with relative frequency : thus, probability becomes a physical property of actual events, e.g. the probability of throwing a '6' with a die is characteristic of that die (and of the manner of throwing). The second school attributes probability to hypotheses, that is, to propositions about events. This view is exemplified by a statement such as this : 'It is probable that it will rain tomorrow.' Naturally, the probability of a hypothesis is usually *based* upon a relative frequency, e.g. the frequency of rain observed for the given date in the past fifty years in London ; but it is not the same thing as that relative frequency. Probability, according to this school, is a logical and mathematical concept ; it is often explained in terms of the logical range of sentences, that is, of the hypothesis and of the evidence.

The frequentists claim on that occasion that their kind of probability is 'objective' and that the logical concept is 'subjective' since it may be taken as referring to the credibility of a hypothesis. The defenders of the logical range theory point out that the frequency interpretation automatically precludes speaking about the probability of single events and that this is needed both in science and in everyday life. It is quite true, in my opinion, that relative frequency is merely a descriptive term summarising past events known to have occurred. If probability is to refer to something unknown—and this is, surely, what we want the concept to mean, at least, in part—then it must be more than a relative frequency.

H. Jeffreys, *Scientific Inference*, 2nd edition, Cambridge University Press, 1957, pp. viii + 236. 25s.

G. H. von Wright, *The Logical Problem of Induction*, 2nd enlarged edition, Blackwell, 1957, pp. xii + 249. 25s.

REVIEWS

These remarks must suffice to characterise the views of the two contending, theoretical schools. The third school is that of the practical statistician who rejects all discussion about the meaning of 'probability' as so much philosophy. He claims to take probability as an undefined notion and in this way to avoid the difficulties. In this he is, I believe, mistaken. It is true that we need not define 'probability' explicitly: definitions are not important in science. But we must give an axiom system and rules for the concept; and this constitutes an implicit meaning. Thus we are still confronted with the same task of finding out what the special interpretation is that we wish to give to the concept of probability.

The three works under review represent, *cum grano salis*, these main schools of thought. I shall start the discussion with von Wright's book since, of the three authors, he is most directly concerned with the philosophical issues involved. We find on p. 1 the definition of induction is given 'as reasoning from known to the unknown'; and it is the logical problem, that is, the relation of evidence to the induced proposition rather than the psychological problem of the discovery of 'invariances and laws' that the author wants to investigate. This brings in at once a question about the justification of inductive arguments, in the sense that such inferences must be justified '*before* their actual experimental verification'. In other words, the attitude taken by von Wright is that of the empiricists and very close to von Mises and Reichenbach. Laws are considered to be universal statements but, all the same, factual. Thus it is natural to interpret 'probability' in terms of the proportion of elements in a class having a certain property, that is, as relative frequency. If inductive method aims at 'connecting two characteristics . . . by universal implication . . .', this move in the epistemological game is forced. Mathematically, the most important theorem of the formal calculus is that of Bernoulli, for without it frequency probability cannot be applied in practice. Professor von Wright formulates it in two parts, in a somewhat novel fashion; but even so he cannot avoid circularity, e.g. 'If the Bernoullian probability of the event is p , then the probability that the event's relative frequency on n occasions will deviate from p by less than an amount . . .' This difficulty is acknowledged by the author when he says 'that in the theorem mentioned *two* probabilities are involved'. A hierarchy of probabilities is needed in order to provide 'a bridge from the realm of probabilities into the realm of empirical frequencies . . .'. Clearly, this indicates the logical collapse of the frequency interpretation: for the concept of probability must be presupposed so that probability may be identified with the limit of relative frequency. The game of epistemology as played by the empiricist *v.* Nature ends in the defeat of the philosopher.

Let me add that this very brief summary does not do justice to von Wright's book; his arguments are often more subtle and acute than

those of his fellow empiricists and range over a wide field. I take as an example the last chapter which is about induction as a self-correcting operation. This feature of inductive reasoning makes it more rational and, thus, superior to other kinds of argument about the unknown. In one sense, this is quite true. It is rational to correct one's mistakes, that is, to learn by trial and error : any method acceptable to science must satisfy this condition. But there is no single, privileged, method superior to any other, i.e. induction, by which this is *automatically* achieved. Such an assertion is of course no more than a weak form of the old Rule of Induction prescribing, *a priori*, the uniformity or regularity of nature without which we are not able, allegedly, to discover specific causal sequences. It has been called 'the faith of the scientist' or, in a more stream-lined formulation, it is the slogan that induction (like honesty, I suppose) is the best policy. Professor von Wright comes very close to saying that it is a tautology, namely, that 'The superiority of induction . . . is concealed in the *meaning* of the policy's goodness'. I would agree with this, for it implies that inductivism—the idea that there is a logical method by means of which new knowledge can be obtained—is, at best, an empty verbalism.

Hume's sceptical attitude to induction is thus supposed to have been overcome. 'To Hume the failure to justify induction seemed the discovery of a serious limitation in man's intellectual faculties.' According to von Wright, this failure says nothing about the world or the human mind : it is merely a consequence of our use of language, and this 'can truly be said to constitute the "solution" of the problem put to philosophy by Hume'. Thus the 'critical' task of inductive philosophy is separated from the 'constructive' task, the one dealing with the meaning of words, the other with the application of logic and mathematics to propositions about the world. This conclusion demonstrates, in my view at least, that induction, in the classical sense of Bacon and Mill, has to be abandoned : there is no logical method by which we can discover whatever is unknown, even though we start from what is known.

Of course, I do not want to suggest here 'that reasoning, properly speaking, cannot be applied to empirical data to lead to inferences valid in the real world'. This suggestion is 'horrifying' to Professor Fisher. I, too, should certainly want to reject it. But the question is : What is the nature of this reasoning ? 'The mathematical attitude to induction' is a phrase that is not sufficiently self-explanatory. We hear that both 'subjective ignorance' and 'objective knowledge' play a part in the probability statement. The knowledge 'refers to a well-defined aggregate, or population of possibilities within which the limiting frequency ratio must be exactly known', while the ignorance 'is specified by our inability to discriminate any of the different sub-aggregates having different limiting frequency ratios . . . '.

REVIEWS

These quotations would put Professor Fisher, together with the majority of practical statisticians, into the camp of the frequentists ; and he might even agree to this interpretation of his views. Yet in fact his attitude is, I think, much more complex. He speaks of the 'semantic difficulty due to an imperfect analysis of words . . . such as the word "probability" itself'. This is brought out more clearly by the method of maximum likelihood which Professor Fisher invented and for which he became famous.

The method is related to Bayes's theorem which Fisher regards with utmost doubt, while Jeffreys, for instance, says that 'This theorem . . . is to the theory of probability what Pythagoras' theorem is to geometry'. The theorem states that the posterior probability equals the prior probability times the likelihood. Statisticians inclined towards the frequency view tend to reject it, since the prior or *a priori* probability seems to bring in a non-empirical element. This is only an unfortunate choice of a term, as Professor Jeffreys points out, and I would agree with him : there is no philosophical *apriorism* here. No knowledge is smuggled in prior to, or independent of, experience. All that is meant by the term is that a mathematical calculus only allows us to derive certain probabilities from others and that these must therefore be given, *initially*.

Professor Fisher interprets Bayes's theorem in a different way by saying that the likelihood is not a probability. Rather 'It is . . . a well-defined quantitative feature of the logical situations in which it occurs, and like Mathematical Probability can serve . . . as a "measure of rational belief" . . . (it) does not obey the laws of probability'. In this way 'each element of probability *a priori* is converted . . . to the corresponding element of probability *a posteriori*'.

These statements bear out, I think, what I claimed, namely that Professor Fisher's view can hardly be called frequentist. This is illustrated by the method of maximum likelihood itself. The method works with an unknown population and a known set of data : the data have a certain distribution, and the problem is to find from them the distribution of the population. For example, we have a machine producing nails and a sample box of 100 nails ; knowing how many of them are defective, we want to find the distribution of defective nails in the manufacturing process. The method consists in assuming hypothetically that the population has a certain distribution, e.g. a normal one, or a Poisson, etc., and then in choosing, as an estimate of the parameter of the unknown population, the value that renders the likelihood as large as possible. Thus another kind of probability, apart from relative frequency, is introduced ; it does not matter what it is called as long as the two kinds are distinguished, and Professor Fisher does so.

The method explains also, I think, the mysterious terminology which Professor Fisher (and others) have used when they speak of the probability

of hypotheses and claim that there is an infinite class of them and that the true hypothesis among them is found by the relative frequency. Now it does make sense to say that it is probable that it will rain tomorrow. But, surely, no one has ever considered a series (finite or infinite) in this simple instance of a hypothesis about tomorrow's weather. We have only to decide between two rival hypotheses: Will it or won't it rain? Other examples, like that of Caesar's crossing the Rubicon, are still less amenable to a frequency interpretation. Professor Fisher says that we may have to consider 'a series, or, more usually, . . . a continuum of hypotheses, one of which *must* be true, and among which a selection may be made . . . and justified . . . by statistical reasoning'. What I venture to suggest here is that the continuum merely refers to the possible values which the mathematical parameter may assume in the distribution representing the hypothesis about the unknown population. Thus, the difficulty reduces to a matter of words. Shall we say that there is one hypothesis which we accept because it is highly probable (as result of the proper choice of the parameter); or do we want to offer an infinite series of hypotheses, each represented by a different numerical value of the parameter, and we select one of them? The method of maximum likelihood and common sense as well would, I think, lead us to the first formulation.

Another idea which colours Professor Fisher's attitude must be mentioned here, namely, that he does not want to make the customary distinction between calculus and application within mathematics. 'The axiomatic theory of mathematics has not been taken very seriously in those branches of the subject in which applications to real situations are in view.' I can sympathise with this practical man's attitude, but it obscures the logic of the situation. It may be artificial to distinguish between formal calculus and interpretation, between syntactic and semantic rules, and so on, when one is concerned with doing mathematics. And it is true that this distinction is relative and depends on the context in which it is made. But some such distinction is inevitable if one wants to talk about mathematics and, for example, explain how to use it. This is the problem here, however. If we are 'formalists' and accept, for instance, Kolmogoroff's axiom set, then the logic of the concept is settled and we need only give the rules of interpretation, say, in terms of the relation between hypothesis and evidence. And when we apply such a theory in practice, we have a mathematical or logical model; and all we need is to see whether it represents fairly the actual situation. If we take the 'intuitionist' view and regard probability—like Kronecker's number—as something intuitively given, then probability theory becomes a kind of physics. We have to show that there exists such an entity as a probability; and when we apply the theory in practice, we have to demonstrate that the entities we investigate are of the right kind.

REVIEWS

The 'intuitionist' attitude of the practical statistician makes it immaterial that we can derive the laws of statistics from the set of axioms, as theorems; they can be accepted only if they are confirmed by experiment. Then we need special criteria to judge whether, and how far, the experiment is relevant to the hypothesis. Think only of the example of throws with a die. If we want to use them as evidence for the truth of statistics, then we must know beforehand that the die is unbiased. If we find that, in a large number of tosses, one side shows up with a frequency less than $1/6$, then we can take this either as falsifying the laws of statistics or as evidence that the die is biased. Which we do depends on criteria *external* to the statistics in question; we need extra knowledge if we are to decide whether or not to accept a statistical result as relevant. This is the reason why practical statisticians were forced to introduce tests of significance. It is a very technical matter, however; and I must therefore refrain from discussing Professor Fisher's very original ideas about such tests.

I have said enough already to show that Professor Jeffreys's views are almost diametrically opposed to those of Fisher. The first thing Jeffreys does is to provide us with an axiom set for the calculus of probability. And he goes on to say that the 'relation between a set of data and a conclusion is called *probability*, and the subject is essentially what is now called a many-valued logic'. Thus, his ideas are close to those of Carnap and the logical range theory, as he himself remarks. Frequency probability, pure and simple, has almost no application according to Professor Jeffreys; and he is severely critical about the logic its proponents have employed. I would agree with him here. However, I find it difficult to accept his view that all laws are probability statements. This is a view which fits in much more with the empiricist tendency of the frequentist. Of course, Jeffreys has a somewhat novel interpretation of what a law is. 'Most of them contain what are called adjustable parameters. For instance, "a mustard flower has six stamens" contains the number 6 . . .'. Similarly, he cites the law of gravitation as an example since the value of the gravitational constant 'has to be inferred from the observations'. The more usual view is, after all, that not all laws, say, of physics, are statistical, e.g. those of mechanics. And the numerical value of any constant occurring in a law is accidental: the constant must even be symbolised by a letter in the proper formulation of the law.

Professor Jeffreys's strictures of the hypothetico-deductive method arise from this interpretation, for he finds the method acceptable but incomplete. The method, he says, 'consists in stating laws for consideration, comparing with observation, and rejecting them if they disagree too badly . . .'. And he continues, 'Since errors of observation exist, any criterion for rejection must be quantitative; and there is no such criterion without at least a partial theory of probability But the statements of the

REVIEWS

hypothetico-deductive method mostly avoid speaking of the probability of a hypothesis and thus fail to deal with the problem of prediction'.

The difficulty comes here, I think, from the failure to distinguish between the law as a statement (or theorem) within a theory and its application in a specific instance. Professor Jeffreys cites with approval a remark (which, he believes, may be due to Fisher) that 'the standard error is just as essential a part of the law as the true value'. Every scientist would admit that, in one sense, this is so; but in another, it is not. Surely, we fondly hope that, with improved techniques, we obtain better data and so can show that our laws fit better than before. Indeed, I would heartily agree that the errors of observation determine the probability of a hypothesis (not of a law, really); but the errors are not a part of what the hypothesis is about.

Professor Jeffreys's views, I fear, are here coloured by a trace of inductivism. If probability is a logical and mathematical concept, then it cannot be *eo ipso* predictive. The world is not logical or mathematical, though our model of it is. It is the scientist who, by proposing a suitable model and by formulating a specific hypothesis on its basis, predicts the future or, in general, whatever is unknown. We see the future as a gamble and so the model we choose is that of the card pack, of the bag and balls, of dice throws, etc. Probability is the mathematical concept that is appropriate for such models.

For a similar reason Professor Jeffreys scornfully rejects the possibility that laws may be disguised definitions, that is, analytic statements (within a theory). 'Definitions of what? If there are n parameters in a law, n sets of values of the variables suffice to estimate them. The information that the law fits for a much larger set of values is ignored.' Now, it is true that many different types of statement are called 'law' in science; sometimes, we speak of an 'empirical law', which happens when we have no proper theoretical basis for it. Such a 'law' may be more like a factual hypothesis (within the usual context of its application). But how about Newton's first law? Where are the parameters in it, what is its probability, and who has ever found an exception to it?

The first edition of Professor Jeffreys's book has served several generations of scientists as a provocative and original introduction to methodology; the second edition will certainly fulfil the same purpose, enlarged as it is and including a new chapter on quantum mechanics. I cannot do justice to the wealth of material offered; and this applies equally to the books of Fisher and von Wright. In conclusion let me only say this.

Probability theory and statistics are still beset by many difficulties, and the main reason is that they are inextricably tied up with the philosophy (or metaphysics) of induction. Thus we find here the argument in terms of a dichotomy that has always been so typical of classical philosophy, e.g. objective *v.* subjective probability, events *v.* hypotheses, frequency *v.* range,

REVIEWS

justification *v.* application, description *v.* prediction, etc. Certainly, one thing that is true is that the meaning of one term of a dichotomy mutually depends on that of the other; thus we cannot resolve a dichotomy by simply cutting it into two pieces as is done by the opposing schools of philosophical thought.

The concept of probability has come to us from two sources—the general notion current in ordinary life and known to Aristotle as *εἰκασία* and the specific notion arising from the games of chance of the seventeenth century. The latter lends itself easily to mathematical treatment, since we have here a very restricted context, simple events, and well-defined rules. The situation to which probability calculus is to be applied is clearly specified, and so we obtain a certain model. It makes prediction almost automatic, since the games—like dicing or the drawing of balls from a bag—are played according to a rule. This brings about the unholy union of philosophical induction with mathematical probability. The union seems to realise one of the oldest dreams of mankind, that is, that mathematics can foretell the future and gain new knowledge for us.

Yet whatever probability may be, it is also a mathematical concept, no different in logical status from any other such concept, say, that of complex number: and no mathematical symbol is, by its very nature, predictive. Probability does no more 'change ignorance into knowledge' than $(a + bi)$ can. Why should a mathematical expression, e.g. the binomial theorem, automatically predict the future when it occurs in statistics, but not do so when used in arithmetic? No—we, the scientists, do the predicting; it is the model we choose; and very often we believe that the future is adequately pictured as the drawing of a ball from a bag, etc. To ascribe miraculous powers to probability is as wrong as to reject its use in scientific method (as some have done). If we can steer this middle course, we shall also manage to free probability theory from the spurious dichotomies that obscure it.

ERNEST H. HUTTEN

THE TELEVISION THEORY OF PERCEPTION

It has been suggested that vision involves a scanning mechanism which works on the same lines as the scanning mechanism used in the transmission and reception of television. Dr Smythies¹ uses this suggestion as the basis

¹ J. R. Smythies, *Analysis of Perception*, Routledge and Kegan Paul, International Library of Psychology, 1956. Pp. xiii + 140. 21s. Smythies gives a brief account of the evidence, pp. 69-71. A fuller account with references is given by W. Grey

for a new scientific version of the old philosophical representative theory of perception, a version which, he says, can be tested by experiment.

1 *Smythies's Sense Data*

When someone looks at a light which is flashing at the rate of six to thirty flashes per second, complicated patterns appear in his visual field. When a television studio is illuminated by a light flashing at this frequency, similar patterns appear on the screen of the receiving set. Now it is known that the patterns which appear on the television screen are produced by the scanning mechanism, and it is concluded that the patterns appearing in the visual field are produced by something similar.¹ Smythies is not concerned with this scanning mechanism as such but rather with the similarities between visual fields and television screens themselves. He concludes from the above evidence that the visual field is a private screen of sense data which *represents* the physical objects of the outer world in much the same way as the television screen represents the physical objects which are being televised. The representatives of physical objects which appear to us on our private television screens are (Smythian) sense data. Smythies sets out to give a scientifically useful definition of the term 'sense datum', one which is free from the incurable vagueness which has pervaded its philosophical use. He does so by starting with an after-image in the visual field. Whatever has spatial relations with this after-image, whatever may be said to be in the same field, counts as a sense-datum. The definition is easy to understand and leaves us in no doubt as to what is to count as a sense-datum. Everyone knows how to obtain an after-image by staring at an electric light. Everyone can put the contents of his visual field into spatial relations with such an after-image. Smythies specifies more precisely in a series of mathematical definitions (pp. 6-7) given in terms of Jordan curves what 'having spatial relations' with an after-image is. If I have a patch of after-image in my visual field the boundary of that patch will naturally divide the field into two areas, one inside and one outside the patch. To say that the outline of the patch 'can be observed to describe a Jordan curve' is a pompous way of saying that it is such a patch. The definitions given of the relations between the patch and the rest of the field reiterate the same information. Their mathematical expression in definitions (I.1-4, pp. 6-7) is a feat of elementary translation from simple into technical language, but as these definitions are not used by Smythies in the development of his theory, we are left with the suspicion that their inclusion is an attempt to cajole us with technicality for its own sake.

Walter in *Perspectives in Neuropsychiatry*, edited D. Richter, pp. 67-78, 'Features in the electro-physiology of mental mechanisms'.

¹ Cf. Grey Walter, *op. cit.*

REVIEWS

2 *How Sense Data are Related to Physical Objects*

If each person is solipsistically confined in this way to his own sense data, which appear to him in his own private space, it is clear that we have to find out how each private microcosm is related to the public macrocosm ; how sense data are related to physical objects. Smythies says (pp. 27-28) that there are two possible theories about this. Either sense data have spatial relations with physical objects or they do not. Theory I is that sense data have no spatial relations with physical objects. This means that each person lives in his own three-dimensional private space, that there is a large number of these private spaces, but that these private spaces are not themselves arranged in any spatial order. Theory II is the more familiar view, popularised by Russell in *Mysticism and Logic*, of the arrangement of these numerous three-dimensional private spaces in a public three-dimensional space, the whole system being a six-dimensional one. There are two possible versions of Theory II. Theory II (a) says that there are 'unsensed psychical entities' beyond the confines of each person's private space, and Theory II (b) says that there are not. What I see when I look around me is, according to Smythies, my own sense data arranged in my own private space. And although my private space extends in principle indefinitely in all directions, in practice I have only a finite section of private space, limited by the furthestmost sense data which I am having. According to Theory II (a) my private sensed space is embedded like an air bubble in water in a larger unsensed space. This surrounding unsensed space is simply a continuation of the private space. It is the private space which I might have if I had sense data which were further and further away. Behind the private screen of sense data lie the unsensed psychical entities. Just as the works of the television set lie behind the television screen so these psychical entities lie literally behind the screen of sense data. The psychical entities are quite literally hidden by the sense data (p. 56). These entities are not in physical space : they are in the same three-dimensional system as sense data are, they bear the same relation to physical space as do sense data themselves. Theory II (b) says that there are no unsensed psychical entities. That there is nothing behind the private screen of sense data, that each private space ends at its furthestmost sense datum. But that the three-dimensional system which is a private space is itself in spatial relation with the three dimensional system which is physical space.

The experiments which will enable us to decide between these theories will consist, he says (p. 125), in detecting the *trans-dimensional* influences between the brain and the sense fields. He characterises how these trans-dimensional influences would appear to a physicist by saying that they would be, for him, forces acting upon the physical universe from the outside, whatever this may mean. He then says, but only says, that two different

systems of mathematical physics would be appropriate to Theories I and II and that these *might* lead to different predictions as to the demonstrable effect of 'mind influences' on systems in the physical world. By 'mind influences' he means factors other than the laws of physics which (allegedly) operate in the brain.¹ The crucial matter of the precise way in which Theories I and II differ from each other empirically and the way in which this difference might be established is relegated to an Appendix of one and a half pages. The content of this Appendix turns out to be, not an indication of what these empirical differences might be, but a mere promise of such an indication.

3 Smythies's Case Against Naïve Realism

A case for a representative theory of perception is a case against naïve realism. If we see representatives of things then we do not see the things themselves. Yet the separate strands of Smythies's argument do not in fact add up to a refutation of naïve realism in quite the conclusive way that he thinks they do.

(i) For example, his method of introducing and defining sense data is neutral between a representative theory and naïve realism. It leaves things exactly as they are. It merely *redescribes* the original situation. It consists in saying that the *aspect* of a seen physical object which can be brought into spatial relation with an after-image is to be *called* a sense-datum. A naïve realist might very well reply 'Go ahead and call them that if you like!' It is absurd to think that this ceremony of renaming of itself gives us an entity, the sense datum, distinct from another entity, the physical object. Smythies speaks as if a change of attitude towards the contents of the visual field combined with a new terminology of itself justifies his saying that there are such things as sense data.

(ii) Since television must, logically must, be representative, and since vision resembles television, he argues (p. 42) that vision must be representative too. But this follows only if television resembles vision in the relevant respect, i.e. in being representative. The argument is circular. There is no trace of incompatibility in the idea that part of the causal story about vision should resemble part of the causal story about television, and that vision should be 'direct'.

(iii) He defends 'The Television Theory' against naïve realism by saying that 'it does not seem very plausible to assert that whereas physiological processes are necessary conditions for our perception of external physical objects, our perception of such objects is nevertheless a process in some manner different from the physiological process' (p. 42). This is an

¹ These ideas are taken over uncritically from Eccles's *The Neuro-Physiological Basis of Mind* (Chapter 8, *passim*.)

extraordinary statement. He seems to be saying that the necessary conditions for perception must resemble the 'process' of perception. On these lines we could argue that since a heart in good working order is a necessary condition for seeing something that the 'process' of seeing something must resemble a heart.

(iv) The evidence from neurology, which Smythies takes as supporting his representative theory, does so only if the question is already begged. For this evidence is derived from two sources. (a) From clinical reports on the way in which the visual field is 'built up' during recovery from certain cerebral injuries (Chapter 2, *passim*). (b) From the patterns which appear in the visual field when the eye is exposed to a flickering light. Now it so happens that each of these kinds of evidence is just as good meat for a naïve realist as it is for representative theorists. The clinical reports on the recovery of vision are taken by naïve realists to be accounts of a gradually increasing access to physical objects. By representative theorists they are taken to be accounts of the gradual building up of the raw materials of vision into sense data. The clinical reports themselves are neutral between these two theories. In the same way, the patterns produced in the visual field by a flickering light are described by naïve realists as private visual sensations which have no more to do with the physical objects which are perceived in normal vision than have the odd effects which anyone can produce by screwing up and opening his eyes rapidly. These same effects are claimed by representative theorists to be the raw materials from which normal vision is built up. The facts themselves give us no way of deciding between these two accounts.

(v) The distinction between sense data and physical objects applies equally well, of course, in the special case of our own bodies. Here, too, we have direct access to a collection of somatic sense data which make up the 'body image' (Chapter 3, *passim*). We have to distinguish, that is, between what we experience of our own bodies (the 'body image') and what we know about them (the 'body concept'). In this connection Smythies cites phantom limbs as providing conclusive refutation of naïve realism. But the naïve realist who would be deterred by phantom limbs would have to be very naïve indeed. His view that we have direct access to physical objects would not be shaken by the fact that certain disorders give rise to persistent illusions.

4 *What Kind of a Theory is a Theory of Perception?*

Smythies does not distinguish between philosophical and scientific theories. He seems to be unaware of the distinction between a philosophical theory which is going to apply to the facts whatever they may be, and a scientific theory which is, by contrast, verifiable, just because it will

REVIEWS

not fit the facts whatever they may be. Even to propose to give a verifiable version of a philosophical theory such as the sense-datum theory shows a complete misunderstanding of what such theories set out to do. To ask the question 'How are sense-data related to physical objects?' within the context of a scientific sense-datum theory is to ask for facts. To raise this same question within the context of a philosophical sense-datum theory is to ask for an elucidation of the relation between the *concept* of 'sense-datum', as defined in terms of incorrigibility, and the concept of 'physical object'. It is not to ask for facts, it is to ask, whatever the facts may be, about the relation between two concepts. It is clear that these two enquiries are different in *kind* and it should also be clear that the entities which are spoken of in both contexts as 'sense-data' are correspondingly different in kind. A scientific (Smythian) sense-datum is defined in the operational way indicated above. The question of the incorrigibility of such sense data is rightly nowhere raised by Smythies. A philosophical sense-datum, on the other hand, is defined essentially in terms of incorrigibility. The former is an empirical, the latter an epistemological entity. They have nothing in common except the name. They are introduced in different ways in answer to different questions. Philosophical sense-datum theories are about epistemological certainty. A scientific sense-datum theory could not and should not be concerned with this. Smythies is deceived by his calling the entities which he introduces 'sense-data' into thinking that they are the sense-data which philosophers have talked about. He is deceived by his own pun.

Smythies takes the philosopher to be a super-scientist who is concerned with the 'synopsis and synthesis of the various branches of knowledge'. If this were the case it would follow that the thing for all philosophers to do would be to learn as many sciences as they could and then set about synopsising and synthesising them. This is not what philosophers do nor what they think they ought to do. There has only been one Herbert Spencer. Smythies says that 'an adequate and comprehensive account of perception' cannot be given in terms of an analysis of ordinary language. Since by an exhaustive account he means an account which includes all the facts known to all the relevant sciences this is obvious. But no-one has ever said that an analysis of ordinary language (whether this is in fact the proper business of philosophy or not) is a *substitute* for any of the sciences. The indiscriminate use of 'theory' in this way enables Smythies to reprimand philosophers for not being concerned with the detail of neuro-physiology, neurology, electronics, psychology, psychiatry, and the having of experiences under hallucinogenic drugs. But the complaint that a philosophical theory of perception is inadequate because it does not take into account the full details of half a dozen sciences can only be made if there is no difference in kind between a philosophical and a scientific theory. It never becomes

REVIEWS

clear in fact what Smythies thinks that a theory, either scientific or philosophical, is. This is one of the major confusions of the book. He says that he is not concerned with a choice between rival philosophical theories of perception. He contrasts them with his own comprehensive scientific theory of perception and assures us that this will take care of the philosophical puzzles about perception. Yet what he describes as philosophical puzzles, such as the question 'Where are sense data?' turn out to be quite specific questions which only arise within, and can only be solved within, the context of a particular philosophical theory, in this case, some version of the sense datum theory.

The book is intended to be a contribution both to the philosophy and the science of perception. Enough has been said to indicate that it is unilluminating on both of these aspects of the subject. The reader is promised a scientifically useful definition of 'sense datum' and a verifiable representative theory of perception. He is fed on promises which are never kept.

JOHN TUCKER

A HEGELIAN VIEW OF COMPLEMENTARITY

In a nutshell, the thesis of the present volume ¹ seems to be this. Kant was a determinist and Heisenberg was not. However, this is not to imply that Kant was wrong since *truth is relative*. A law, whether deterministic or antideterministic, is valid *only* within the 'mathematical framework' accepted at the time. This relativism, Cassirer seems to imply, is not an *ad hoc* attempt to save Kant, but a direct consequence of the necessary generalisation of Kantianism, intended to render it applicable to later developments in physics. The generalisation is necessary because of the invalidity of the alternatives to Kantianism, i.e. a theory of induction, and the view that theories are used for the economy of thought (positivism ²). The only choice then is to generalise Kant's *apriorism* so as (a) to include the concept

¹ Ernst Cassirer, *Determinism and Indeterminism in Modern Physics, Historical and Systematic Studies of the Problem of Causality*. Translated by O. T. Benfey, with preface by H. Margenom. Yale University Press (London: Oxford University Press) 1957. Pp. xxiv+213. 40s. (The original edition was published in 1936.)

² Cassirer attacks Mach's Positivism. His own view is similar to Duhem's positivism. Cf. my 'Duhem versus Galileo', this *Journal*, 1957, 8, 237-248, esp. p. 245

of statistical laws in the Kantian concept of causality, and (b) to admit that the *a priori* 'mathematical framework' is, contrary to Kant's view, not fixed and ultimate, while insisting with Kant that it is a necessary condition of all scientific experience. This *relativist theory of truth* is Hegelian and the point of this review is to show that this is so in spite of Cassirer's professed Kantianism.

Cassirer's notoriously apologetic attitude towards Kant does not permit him to state a difference between Kant's and his own views as sharply as it is done here. Moreover, he uses his departure from Kant's views mainly as a defence of Kant, though admittedly in a very Pickwickian sense of defence. He claims (p. 123), that the law of causality still holds if it is formulated as 'if we can measure precisely the initial conditions of a system, then we can predict accurately its behaviour', but the antecedent, he points out, is, according to Heisenberg, never satisfied. Causality thus formulated holds vacuously. Cassirer evidently felt that this emptiness is far from being satisfactory. This is, of course, not to mention that we can formulate the law in a way which makes it false, e.g. 'the behaviour of any system is predictable, in principle, with any degree of precision'. He therefore also asserts that (although the world seems to us to be less well ordered than it seemed to Kant) at least the world is not entirely chaotic and that there is some kind of 'causality' even in statistical laws. However, Cassirer noticed the verbal character of his defence (viewed as successful in Margenau's preface, p. xi). Therefore, besides showing that Kant was partly right even by our own standards he asserted that even if Kant was wrong by today's standards he was right by yesterday's standards.

This however, in so far as it is acceptable, is quite trivial. We know that a Newtonian had, at least *prima facie*, some justification for being a determinist. The point is that since Newtonianism is not in agreement with facts determinism has no leg to stand on. Cassirer's assertion that the validity of a law is determined only relative to its 'framework' will not help, because we must consider the question of the truth, or validity, or acceptability, or what have you, of the 'framework' itself. To put it differently, why was the Newtonian 'mathematical framework' abandoned? Cassirer insisted that it was abandoned not because of lack of agreement with facts. We could always account for unpleasant facts, he argues, by *ad hoc* adjustment of minor points in our theory leaving intact the major ones, those which he called the 'mathematical framework'. Newtonianism was superseded, we are told, because of the greater simplicity of Einstein's theory. The demand for simplicity is an *a priori* demand, not an empirical fact. Yet, this argument does not help either. Quite generally whatever else is contained in our conditions for acceptability, if these are universal, they are ours as well as Kant's or Newton's. We know today that Newtonianism is unacceptable, because we know of facts which

REVIEWS

were not known in the days of Kant or Newton. Thus, even if we encumber our discussion by all kinds of sophisticated arguments (and these are not wanting), the fact remains that it was the discovery of new facts which somehow led us to reject Newtonianism. This may be illustrated by the situation in which two 'frameworks' explain facts adequately. In such a situation we try to design a *crucial experiment* which will enable us to overthrow ¹ one of them.

Cassirer's theory does not explain even simpler cases such as the unacceptability of a theory identical with that of Newton in all detail save in its assumption about the number of the dimensions of space. In Cassirer's sense a four dimensional Newtonian 'mathematical framework' is a perfectly respectable 'mathematical framework', while of course no-one would have considered its acceptability. Admittedly, four dimensional Newtonianism is less simple than the ordinary three dimensional Newtonianism. This, according to Cassirer, is a minor defect. He cannot explain why Newtonianism was once accepted while four dimensional Newtonianism was not. The fact that a theory was once accepted is for him not something to be explained rationally, but something we must accept as given. He admits in the introduction to the present volume that in his *Substance and Function* (1910), 'classical physics was accepted as valid and uncontested' because at that time most physicists accepted it. One would expect him to have learned from experience and accept nothing 'as valid and uncontested', especially in a volume which propagates relativism. On the contrary, however, his relativism means that any view which enjoys universal agreement is true. Thus, he consistently accepts 'as valid and uncontested' quantum theory as he understands it, in spite of his view that quantum theory is inconsistent. (See Index, art. 'complementarity'.)

The assertion that Cassirer accepts the majority view 'as valid and uncontested' can be denied because while physicists usually consider quantum theory as consistent, he views it as inconsistent. This apparent disagreement with the majority view is the result of Cassirer's misinterpretation of Bohr's complementarity, or at least of Bohr's intentions. Cassirer suggests that Bohr's principle of complementarity asserts that quantum theory incorporates both the wave picture and the particle picture. It is because he so misunderstands the principle, and because he accepts anything that most scientists accept, that he is ready to justify Bohr—as he misinterprets him—by claiming that 'conflicts' are fruitful and need not lead to 'skeptical renunciations' (p. 174). Moreover, for this purpose he squeezes, even from Planck and Kant, permission for 'antitheses' in science (p. 80).

It goes without saying that Kant and Planck cannot be blamed. While 'light is both continuous and discontinuous' is plainly a contradiction,

¹ Cf. K. R. Popper, *Logic of Scientific Discovery*, London, 1958, Section 15, and his *Open Society*, third edition, London, 1957, vol. 2, pp. 12, 266, 364

REVIEWS

Kant's statement which Cassirer quotes concerns conflicting *tendencies*, and tendencies can contradict each other only in Hegel's dialectics, not in ordinary 'merely formal' logic. Planck, similarly, never asserted that since time reversibility and time irreversibility are incompatible properties the conjunction of Newton's dynamics (which is reversible) and classical thermodynamics (which is not) is a contradiction.¹ It must be pointed out that Bohr never claimed that quantum theory is inconsistent. He was worried about the inconsistency between two *intuitive presentations* of an abstract theory, and he wished to argue that even these two intuitive presentations do not clash because of Heisenberg's principle of indeterminacy. Thus, Cassirer had not the slightest reason to claim inconsistency of quantum theory on the authority of Bohr. Even if we grant, counter to Bohr's view, that the wave picture and the particle picture are in 'conflict', this will only imply the breakdown of an attempt to comprehend intuitively the (consistent) abstract theory. Yet Cassirer combines wave mechanics with the wave picture, and matrix mechanics with the particle picture, implying (e.g. p. 212, line 5 from bottom) that wave mechanics and matrix mechanics *contradict* each other because the statements 'electrons are waves' and 'electrons are particles' do. The fact is, however, that matrix mechanics is deducible from wave mechanics, and that the wave equation can be viewed as a means for transforming matrices into other (diagonal) matrices.²

Thus, Cassirer's reliance on Kant's, Planck's, and Bohr's authority is based on an error. None of these thinkers believed that contradictions in a theory need not lead to its rejection. The fruitfulness which Cassirer described

¹ Irreversibility of thermodynamics was pointed out as an argument against those who hoped to *reduce* every physical theory to Newton's theory of gravitation, or at least to a theory of Newtonian character, like, say, a theory of the form of a Hamiltonian equation. As Maxwell already pointed out, since all Hamiltonian equations are invariant to the transformation which reverses the time order, and since thermodynamics is not invariant to this transformation, one cannot deduce thermodynamics from any Hamiltonian. ('Maxwell's demon' is another argument for the same contention, namely that thermodynamics is irreducible to causal mechanics: while the demon violates thermodynamics he does not violate mechanics.) Planck's major fight was that thermodynamics should be accepted in its own right (see his *Scientific Autobiography*). In brief, irreversibility is an argument for logical independence, not for an 'antithesis', or a 'conflict', or a contradiction.

² Schrodinger claims to have shown that his wave equation is equivalent to matrix mechanics. The greater generality of his equation is shown by its applicability to a-periodical cases (which are not soluble by matrix algebra) like collisions and potential barriers. However, perhaps this is only evidence for the reviewer's reluctance to assert that the relation between wave and matrix mechanics is that of *mutual* deducibility. For the sake of the present review the assertion of compatibility between the two formal theories is enough.

REVIEWS

was not of self-contradictory theories but of free discussion between people whose opinions contradicted each other. In a free discussion one party tries to *refute* the view of the other party by finding a contradiction in it or between it and a statement of fact. If contradictions were accepted discussion would be futile since the refutation will not lead to a change of view. The fruitfulness of discussion is conditioned by our willingness to get rid of contradictory views, or of views contradicted by accepted observational reports¹. Cassirer approved not of conflict between ideas of two opponents, but of conflicts within the accepted 'framework'; that is to say, he approved of accepting inconsistent views. Of course it is his right to consider himself a Kantian; but one may argue that he was more of a Hegelian. Indeed, a Hegelian may accept determinism and indeterminism as two expressions of the human intellect in different periods, as parts of the *Zeitgeist*, as true each in its own *Zeit*. But a rational thinker should state what *arguments* made him change his views and defend those arguments—as Kant did. Whether Cassirer was a Hegelian or a Kantian, the present reviewer found in the whole volume not a trace of such a rational discussion of Cassirer's change of view from determinism to indeterminism. (However, it is odd that Hegel's name is never mentioned in a volume which so often seems to allude to him.)

Cassirer does not discuss his relativism and 'conflicts' in detail, but they recur in crucial passages, and especially in the decisive parts of the main sections of the book, section 7 on classical statistics, and section 9 on quantum theory, as well as at the end of the volume. The significance of these sections may be seen from the fact that Cassirer hardly discusses indeterminism anywhere else in the book whose title is *Determinism and Indeterminism in Modern Physics*.

Of the remaining eleven sections, one endorses the frequency interpretation of the probability formalism, two discuss the history of atomism, one discusses ethics², and seven are devoted to classical views. Readers of this *Journal* may expect to find in this book a thorough discussion of modern physics. It should be noticed, therefore, that the discussion of twentieth-century physics occupies a surprisingly small portion of the book. The major part of the present volume is historical. It contains too many

¹ K. R. Popper, 'What is Dialectic?' *Mind*, 49, 1940, p. 403 sqq.

² In his section on ethics, Cassirer shrugs off the statistical law in order to assert freedom just as Kant had shrugged off the deterministic law in order to assert freedom. Cassirer's act is perhaps less spectacular than Kant's, but it is at least more consistent. The reviewer considers this a great improvement, even from Kant's own point of view—assuming, of course, that Kant was greatly worried about his own inconsistency. But Cassirer views the situation differently, allowing for Kant's moral theory even in the presence of determinism. Here Cassirer is indeed consistent: Kant of the present volume does not allow himself to be worried about 'antitheses'.

REVIEWS

discussions for a short critical survey to go into. However, a few general remarks on them may be relevant. Firstly, they may be helpful for a research worker, for they are often very interesting; often containing almost unknown yet important information, and sometimes concern problems which are touched upon almost for the first time. Secondly, almost all the discussions are somewhat unreliable for the general reader for they are often much too brief to be valuable or even understandable. Only when Cassirer sticks to one problem for more than a page there is hope of seeing his point, though even the length of a page is not enough if the rich history of the 'union' between the calculus and physics is compressed into it (p. 161). It is doubtful whether many readers will flatter themselves that they understand Cassirer's occasional Greek, Latin, and French, and that they are sufficiently well informed to comprehend remarks which involve so many terms and ideas mentioned by Cassirer with little or no explanation. These include transcendental analysis (p. 17), Russell's theory of types (p. 30), Maxwell's demon (p. 77), Boltzmann's H-theorem (p. 79), 'energetic' thinking (p. 117), the Eleatic critique of becoming (p. 144), occult qualities (p. 146), stereochemistry (p. 147), transcendental logic (p. 166), Dedekind's definition of the irrational number (p. 169), and Wilson's photographs of the α - and β -rays (p. 182). Finally, apart from the dazzling effect of the frequent transitions from modern to ancient science and back, as well as between physics, mathematics, and philosophy of different periods, Cassirer hardly bothers to explain the purpose of his jumps, so that the volume lacks continuity.

In considering the volume as a piece of connected thought it is pertinent to ask: how much can the author get away with? Surely Cassirer did get away with the present volume, whereas Hegel did not get away with his notorious *Naturphilosophie*. Yet one may wonder how far from Hegelian confusion is the following not untypical quotation (p. 169): 'Cantor's foundation of the theory of sets and Dedekind's definition of irrational number could be considered as a final solution of the problem [of the continuum?], for *solely by means of pure thought a continuum was established which was truly commensurate with that of observable magnitudes and which was able to express the latter in all its individual features.*' (italics mine.) Each phrase in this quotation is a testimony to Cassirer's inadequate mathematical presentation while the quotation as a whole shows only his inadequate presentation of what is required from a solution of a mathematical problem. Cassirer admits almost explicitly that the discussion from which the above quotation is taken is a digression from his discussion of continuity in physics. Moreover, he discusses continuity in physics for no better reason than that waves are continuous and that he ought to comment on the wave-particle duality. Indeed, he ends the chapter on continuity with a jump into quantum theory, asserting that the

'conflict' between continuous waves and discontinuous particles need not lead to 'skeptical renunciations'.

It seems clear why Cassirer could get away with such a confusion. He was a humanist and a universalist, he was interested in mathematics, in the social sciences and in the natural sciences, besides his being a Kant scholar, a historian of ideas, and an epistemologist. We revere the ideals of universalism and admire a person who shows some competence as a universalist, even if we admit in our hearts that in each field he will be beaten by any specialist. It is just because the present reviewer shares these sentiments that he is reviewing a book which it is hard to recommend. Our ideal should indeed be universalism—but not at any cost. If the result of attempts to achieve a more universal knowledge overlaps with Hegelian confusion then we should try a new solution to the problem of specialisation. However, we must refrain from taking the present volume as a model whatever views we may hold concerning other writings by Cassirer.

The volume is prefaced by an essay of Margenau which brings it up-to-date. Margenau seems to be much more informed about quantum theory, but there is hardly anything new or interesting in his preface, save his reference to the so-called von Neumann proof not as a proof but as 'very good arguments'. Indeed, Bohm's successful attempt to create a consistent theory which (a) asserts that electrons possess precise positions and precise momenta simultaneously, and which (b) contains Schrodinger's equation, seems to indicate that von Neumann's 'very good arguments' are not conclusive after all, that they do not consist of a proof that a theory which has these two characteristics must be inconsistent. (That Bohm's theory is *factually* false or at least very much *ad hoc* is, of course, entirely irrelevant, since the question is whether von Neumann's arguments consist of a proof that a theory like Bohm's must be *logically* false). Anyhow, it is a pity that Margenau only indicates a change of opinion, that he does not mention that prior to its actual occurrence Bohm's success was considered (at least by the majority of physicists) to be logically impossible. He should have attempted at least, to indicate what von Neumann's 'very good arguments' are, if not to explain and defend them.

However, Margenau is probably right in asserting that Bohm's theory would, if it be experimentally corroborated, only strengthen Cassirer's thesis (p. xix). What would refute Cassirer's thesis is a proof that classical physics already implied indeterminism. In his bibliography, Margenau mentions works by Popper and by Landé in which these philosophers claim to have argued very strongly for indeterminism on the basis of classical views only. But these works are not even alluded to in the up-to-date preface.

J. AGASSI

REVIEWS

Arthur Stanley Eddington. By A. Vibert Douglas.

Thomas Nelson and Sons, Edinburgh, 1956. Pp. xi + 207. 25s.

ONE day early in September 1939 I was talking to the late E. A. Milne in his study in North Oxford when he suddenly confided to me that he had recently bought a radio set and had been listening to broadcasts from Germany, as well as our own news bulletins. He then startled me by remarking that it had recently struck him that here was 'the voice of Eddington in the political sphere'! Conscious of my astonishment, he hastened to explain that all he was saying was that the object of these broadcasts was propaganda for a given point of view rather than the dissemination of truth. I went away marvelling at the superb parochialism of Cambridge men.

Despite an element of truth in his rival's peculiarly expressed charge, Eddington was one of the great men who, in the first four decades of the present century, caused many students of science to regard Trinity College as the centre of the universe. His pioneer work on the internal constitution of the stars is still generally regarded not only as his greatest contribution to knowledge but as placing him in the front rank of twentieth-century astronomers the whole world over. As Milne himself wrote shortly after Eddington's death, in this field 'it was Eddington who brought it all to life, infusing it with his sense of real physics', and of his supreme 'capacity to create interest to sweep away the cobwebs of formalism, to substitute real stars for mere "Gaskugeln" there can be and will be no two views; Eddington will always be our incomparable pioneer.'

Like Newton, however, Eddington was by no means a typical modern man of science, for he too was a voyager through strange seas of thought *alone*. Like Newton he was a synthesist and also a mystic. Even in the years when he was laying the foundations of theoretical astrophysics he did not lose sight of the grand objective—Fundamental Theory. The philosophy which Eddington associated with this theory has frequently been subjected to acute and damaging criticism, notably in the brilliant and witty lecture delivered in 1954 by Professor Dingle, but the symbolism of the theory and the extraordinary calculations of the constants of nature suggested by this symbolism are still **capable** of exercising a hypnotic fascination, despite frequently expressed charges of 'fudging' and 'numerology'.

Eddington's life and work therefore present a tremendous challenge to the biographer, and Miss Douglas must be congratulated on her courage. Even if the definitive assessment of the theory which Eddington regarded as the crowning achievement of his life has not, and probably cannot, yet be made, she has provided us with a simplified but absorbing account of his principal intellectual adventures.

I was pleased to see that her book is dedicated to my former mathematics master, C. J. A. Trimble, who was Eddington's most intimate friend for

REVIEWS

forty years. Mr Trimble had gathered much information about Eddington's family and his personal life which Miss Douglas gladly acknowledges. Eddington was a man of great reserve, and like many famous scientists lived a life of marked simplicity. This is one side of the picture, but intellectually his environment was that of many of the keenest minds of his age. Miss Douglas informs us that she has endeavoured to provide both portraits and 'the stereoscopic merging of the two into one faithful likeness of a great man'. It is no disparagement of her efforts to point out that one of the pleasantest features of the book which contributes significantly to this end is the beautiful selection of photographs, notably Eddington as a schoolboy, as Senior Wrangler in 1905, at the Kepler Monument in 1928 and finally after he had been elected O.M.

After three introductory biographical chapters taking the account down to Eddington's appointment as Plumian professor in 1913, there is a chapter on his work at Greenwich on star-streaming (discovered by Kapteyn in 1904) which culminated in his first book, *Stellar Movements and the Structure of the Universe*, published in 1914. In view of the philosophy of science which Eddington developed later, it is fitting that we should be reminded that he began as an observational astronomer.

A further biographical chapter is then followed by one on Eddington's contributions to Relativity. Eddington's interest in this subject—he was the first authoritative exponent in this country of Einstein's General Theory—was ultimately to be of far greater significance for him than his work in astronomy and astrophysics. For the development of his philosophy of science it was crucial, as Miss Douglas clearly indicates.

One of the most enthralling chapters in her book is that on Eddington's contributions to astrophysics, and her account of the Homeric debates with Jeans at the Royal Astronomical Society is fascinating. This is followed by further biographical chapters, including one on Eddington's religious beliefs, and then by a long chapter on Fundamental Theory. Miss Douglas has given us a fair summary of Eddington's ideas on this subject, even if in places it is somewhat superficial (perhaps unavoidably so). Her choice of quotation to head the chapter is particularly felicitous—four bars of Schubert's *Unfinished Symphony*.

A final word of praise must be given to the printer for the excellent way in which the book has been set up and to the publisher for making it possible for this absorbing biography to appear in print.

G. J. WHITROW

REVIEWS

Sound and Symbol. By V. Zuckerkandl (translated by W. R. Trask).
Routledge and Kegan Paul, London, 1956. Pp. 399. 32s.

THE author's aim is to show that the study of music, though not providing fundamental information unavailable from other sources, gives useful clues for the elucidation of many basic philosophical problems, especially those of time and motion.

Physics depends on the primacy of vision and is concerned with visual-tactile space and spatialised time: time in music is pure 'duration' in Bergson's sense; and tonal space, though essential to music, is non-metric. Musical time, unlike physical time, is causative in itself. Tonal forces are objective but immaterial. 'Tone . . . is the only sensation that encounters us not as the property of a particular bodily-spatial thing. We see blue flower; we touch smooth wall; but we hear tone—not sounding string.' Gestalt psychology, so closely allied to Bergson's philosophy, has been preoccupied with visual, as opposed to aural gestalten because it has been afraid of St Augustine's old problem of the reality of past and future, which must be included in the temporal gestalt. What a pity that there is no tenseless form of the verb 'to exist': it would have saved the author so much argument!

While there can be no doubts about the interest of this book, one must reluctantly conclude that the author has not proved his theses. Where has he failed?

First, though Humpty Dumpty said, 'When I use a word, it means just what I choose it to mean; neither more nor less': yet it is not fair to give words several different meanings in the same argument. The words 'force', 'space', 'motion', 'time' and others are used with so many different connotations that the arguments become confused. To give a few illustrations only, for the use of 'force': 'Without forces, no bodies; but equally, without bodies, no force'; 'In the outer world there are forces active, whose activity transcends the physical'; 'We see forces in colours in the same sense in which we hear forces in musical tones'; 'Force is not an "operational concept"—something that we as thinkers add to the observed phenomenon in order to explain it . . . '.

Secondly, 'tone' (in his sense) is perceived in a way not so different from that of other sensations as the author believes. 'Music is motion, a continuous progression, and yet objectively, nothing but stases and gaps are given in it. . . .' This is necessarily mechanically true of most Western musical instruments, other than strings; but these are serious limitations and, as the author himself is clearly aware, the *portamento* of the human voice rebels against the rigidity of tones and semitones. The author believes that duration is truly perceived only in musical tones. 'To be sure, every sensation possesses duration; the pressure of the weight in my hand lasts

REVIEWS

while I hold it. . . . But here, the duration is nothing but a neutral base. . . . I do not touch duration in the heaviness.' Yet it has been shown that, if I squeeze an elastic body in one hand and, simultaneously, a viscous material in the other, my judgment as to which is 'firmer' will depend, within wide limits, on the time of the squeezing. Would Husserl (quoted by the author to support his distinction) have included such things as 'time objects'?

Thirdly, too many key experiments are neglected. To quote but a few: Hoagland, Piéron, and others, on the effect of body temperature on the sense of time; Piaget on space-perception; Michotte on perceived motion and apparent cause-and-effect; electro-encephalography in general; effects of mescaline on time-sense; and much work on the 'specious present'. Fourthly, many statements starting 'we hear . . .', 'we know . . .' are so subjective as to be meaningless, certainly to one not highly trained in Western music. Sound, as an air vibration, is more diffusely accepted than is light, but experiments with colour organs suggest that colour appreciation, as opposed to vision, may not bear a relation which is so very different in its mode of perception from that of tone, as opposed to the mere hearing of sounds.

Finally, the author is, in some places, extraordinarily naïve, e.g. How can God be 'timeless'? He would have to renounce music! Associationism, a very dead horse, is flogged with the wrong whip, and there is much confusion over 'the meaning of meaning'—of words and melodies.

Yet, in spite of its failures, this is a thought-provoking book which will make most of us realise how much more we (and the author) ought to read before we venture into some of these thorny fields.

G. W. SCOTT BLAIR

An Introduction to Scientific Research. By E. B. Wilson, Jr.

McGraw-Hill Book Company, 1952. Pp. xi + 375. £2 5s.

BOOKS on scientific method which might appeal to the research worker as a laboratory guide because of their immediate practicality are rare. It is interesting to reflect that most books on scientific method fall into two classes. The first usually tend to appeal (and this is unfortunate) more to the philosopher of science than to the practical scientist. Such books describe the processes of observation, induction, and deduction in logical terms, and treat science as an ordered system of statements whose truth values, relationships, and existential status are discussed. The second type of book frequently concentrates on the process of induction from a logico-mathematical standpoint and consists of a sophisticated attempt to justify the truth of statements arrived at inductively.

REVIEWS

Here is a book, several years old, which seems to have received little attention in this country. This is a pity, for it covers both fields mentioned and includes, as well, a much wider variety of topics of immediate use to the laboratory worker. The fields covered by the book range from the choice and statement of research problems to a final chapter on the reporting of the results of research. A useful chapter is devoted to reference books in various subjects and a list of abstracting, indexing, and review journals. This is followed by an account of scientific method. The account given is brief and Baconian. Perhaps the author will have an opportunity of revising this in the light of more recent publications on the hypothetico-deductive method if a new edition is ever contemplated. Other chapters cover the design of experiments, classification, sampling and measurement, the analysis of experimental data and the errors of measurement. Two essentially mathematical chapters on the technique of deduction and on aids to calculation are added. Each chapter concludes with a comprehensive bibliography.

There is little which is original in the subject-matter of the book, but, for the most part, the references given are sound and the method of presentation is clear. Professor Wilson is to be congratulated in writing such an excellent reference work as are also the publishers for the excellent quality of the production.

R. J. F. WITHERS

Methods and Criteria of Reasoning. An Inquiry into the Structure of Controversy. By R. Crawshay-Williams.

Routledge and Kegan Paul, 1957. Pp. viii + 296. 32s.

In his new book Mr Crawshay-Williams brings before us a theoretical problem of great importance, the problem of how in a special class of cases 'we use language as an instrument of reason' (p. 3). His book derives value not only from the importance of the problem, which has suffered neglect for too long, but also from the detailed, careful, and orderly analytical treatment it receives. The sub-title which the book bears arouses interest, but as a description of the subject investigated, it is too general. For the investigation addresses itself, not to every type of controversy, but rather to a special, ill-understood type of disputation met with particularly in philosophy and in the more theoretical parts of the sciences. This kind of disputation he aptly characterises as 'intractable', a characterisation which might usefully have entered into the sub-title: *An Inquiry into the Structure of Intractable Controversy*. The style throughout the book, which is impressive for its wide range of material, is easy and engaging and often vivid.

REVIEWS

The author tells us that his 'original motive for the enquiry was a desire to find out why certain kinds of theoretical and philosophical controversy are so oddly intractable' (p. 3). Disputes such as those he cites, e.g. whether time is real, whether in the process of division the amoeba dies or only loses its identity, whether it is possible for a proper part of a whole to be as great as the whole, whether viruses are living things, and whether mathematical statements express thoughts, are disputes which apparently are or can easily become 'inherently inconclusive'. These may seem to many people to be like disputes over propositions for which there is as yet no conclusive evidence; but this likeness exists only in appearance, not in fact. Crawshaw-Williams makes the actual gulf between them visible at the outset by remarking the fact that in the case of the inconclusive disputes in philosophy and elsewhere argument continues without successful termination in the face of the stated evidence, whereas in the usual case of a disagreement over the truth-value of a proposition for which there is insufficient evidence further argument is considered futile and is suspended, or if the evidence is complete the argument comes to a successful conclusion. His own words are graphic and deserve quoting: 'The more purely scientific and factual the question involved, the more often this last is in fact what happens in case of disagreement; if the scientists have not enough evidence, they go out and look for more; they do not go on arguing. But the more theoretical and philosophical the question, the more often it is argued about interminably, even—or perhaps especially—by people who have been carefully trained in the technique of logical thought' (p. 3).

What is required both to come to an understanding of the nature of this important class of questions and to devise techniques for clearing them up is a precise and methodical investigation of methodology, the underlying idea being that this will bring to light factors the neglect of which causes fruitless debate and the specification of which yields results. The core theory developed in explanation of the nature of this class of questions roughly summarised is the following. The statements which the questions revolve on are of the kind that are not testable 'by appeal to definitions nor by appeal solely to the facts nor by appeal to the truths of logic' (p. 16) and the disputes to which they become subject are 'unfactual' (p. 15). The characteristic nature of such statements is indeterminacy with respect to context or purpose, and it is this indeterminacy which prevents the statements from being established and the controversies over them from being settled by show of fact. When these factors of indeterminacy are given attention and specified the indeterminacy vanishes and the statements become amenable to verification. In the writer's own words: 'If a question of the form "Is it correct to say that A is B?" is discussed in terms which exclude its being settled either by appeal solely to the facts or by appeal to logical deduction, then it will give rise to an inherently

REVIEWS

inconclusive controversy unless any answer put forward is related to a specific context or purpose' (p. 90). This thesis is worked out with a wealth of detail and is applied to recalcitrant problems in philosophy, biology, mathematics, physics, and to other fields. As a succinct application of the thesis, consider the question (p. 125): 'Is there no difference between a proposition of logic and a proposition of pure mathematics?' when asked and debated by people who know both logic and mathematics. This resists an answer which everyone will accept until it is explicitly related to a purpose: 'Is there a need to differentiate between a proposition of logic and one of pure mathematics?' Read in the context of purpose, i.e. in relation to the statement: 'There is no need to differentiate between logic and mathematics (or it is notationally uneconomical or inefficient or inelegant to do so)', the truth-value of the statement 'Mathematics is really logic' becomes accessible.

Crawshay-Williams's theory will undoubtedly have great appeal for many people, and especially for those who have become uneasy about philosophy, the great preserve of inherently inconclusive disputes. This reviewer, however, is doubtful of the correctness of the theory, for the following reason. The underlying premise throughout the book is that chronically debatable statements *have truth-values*; and the theory is designed to explain what it is which prevents agreement about the truth-values and how they are to be established successfully. But the truth-value of a statement is independent of the purpose of making the statement, and it is difficult to see how failing to make the purpose explicit could, by itself, possibly prevent the truth-value from being known. If the disagreement is *actually* about the truth-value then only more evidence, not mention of purpose, is relevant to the establishment or disestablishment of the statement; and if more evidence is of no avail, while mention of purpose serves to remove the disagreement, then the divergence cannot be about the truth-value of the statement. Crawshay-Williams maintains that his argument 'has at no time been that truth is' relative to purpose; it has been that the truth of an *indeterminate statement* is relative to purpose' (p. 255). But if an indeterminate statement *has* a truth-value, then it too must be independent of the purpose of the statement.

MORRIS LAZEROWITZ

Einführung in die symbolische Logik, mit besonderer Berücksichtigung ihrer Anwendungen. By Rudolf Carnap.
Springer-Verlag, Vienna, Pp. x + 209. 47s. 6d.

READERS of Professor Carnap's *Abriss der Logistik*, now long out of print, and all who are interested in the applications of mathematical logic, will

welcome this book. But it is much more than a reissue of that earlier book. It is, as we are told on the dust-cover, 'a completely new work based on the present-day state of the science': we are also told that symbolic logic has become a basic science of the first rank, the importance of which is today highly estimated, especially in the Anglo-Saxon countries. This must refer to North America because it is hardly true of Great Britain. In this country it is held that logic is useless for science because Francis Bacon said so and, since we are a conservative people, suspicious of new things, it will require more than a century of De Morgan, Boole, Jevons, Venn, Whitehead and Russell (not to mention Pierce, Frege, Łukasiewicz, Tarski, the author of this book, and many others) to overcome the influence of the great Lord Chancellor. But let us try to forget the Lord Chancellor for a little while and look inside Carnap's beautifully produced book. It is divided into two parts. The first tells us all we need to know about symbolic logic in order to begin to use it in natural science. Everything is here explained very thoroughly, carefully, and clearly. The second part gives actual examples of applications in the form of axiom systems belonging to mathematics (set-theory, arithmetic, topology, and geometry), physics, and biology.

In Part I two symbolic languages are constructed: language A and language C. Strictly speaking these are not languages but skeletal frameworks of languages which become languages only when we assign an interpretation to certain of the symbols. In order to construct a language for dealing with a given domain of investigation we require, according to Carnap, (1) names for the objects which are regarded as individuals in this domain and (2) designations—called predicates—for the properties and relations which are exemplified in this domain. The language A contains the signs necessary for the construction of compound statements, e.g. negations, conjunctions, disjunctions, etc.; and when variables which can have statements substituted for them have been introduced, the principal formulas of the sentential calculus, which govern the manipulation of such compound statements, can be given. The next step is the introduction of signs which can have individual names substituted for them, so that, with quantifiers, expressions corresponding to the use of 'all' and 'some' can be constructed. Then predicate variables and the notion of higher order predicates are explained. These additions bring with them further lists of formulas involving their use. At this place (p. 38) Carnap explains the important distinction between intension and extension. A sentence has a proposition as its intension and truth-value (true or false) as its extension. An individual name has an individual concept as its intension, an individual object as its extension. For a one-termed predicate the intension is a property and the extension a class. Language A is completed by provision for the notions of identity, of cardinal number, and name-forming functors.

REVIEWS

The language C contains all the means of expression of language A with the exception of sentential variables, which are rarely needed for the formulation of scientific theories. It also contains linguistic devices which enable us to give briefer and clearer formulations of scientific statements. In this section we learn to operate with predicates, and to construct statements involving compound predicates, without the use of individual variables. No distinction is made between a predicate as a name of a property and the same sign as a class designation (languages A and C are purely extensional), and so no sign of class-membership is used (p. 96). This part also contains an admirable account of relation-theory, together with sections on the λ -operator, descriptions (which are not much used), finite and infinite sets, and continuity. Between the parts devoted to languages A and C is a part called 'Language System B' in which are given the syntactical and semantical rules for languages A and C, and such topics as definition, proof and derivation, and the system of types, are explained.

Part II, in addition to giving examples of axiom systems belonging to various sciences, also contains expositions of the distinction between thing-languages and co-ordinate languages and their various forms, quantitative concepts and the formulation of laws, and several pages devoted to a general explanation of the axiomatic method.

All sections of the book are provided with illustrative examples of the methods discussed and exercises for the reader. There are copious bibliographies and indexes. An enormous amount of useful information has been packed into these 209 pages. Of all recent books on mathematical logic this is by far the most convenient from the point of view of the student of science. From the point of view of the philosopher of science, the axiomatised theories here presented offer valuable material for investigation. The following misprints have been noticed : P. 9, line 12, 'schein' seems to be a misprint for 'sein'; p. 82 last line but one, a subscript 'j' appears to be missing after a Gothic S; p. 114, line 20, the letter 'x' in the quantifier should be 'v'; p. 127, line 8, 'x' seems to be a misprint for 'a'. An English translation of this book is in preparation.

J. H. WOODGER

The Background of Astronomy. By Henry C. King.
Watts, London, 1957. Pp. vi + 254. 18s.

ONLY during the last two or three centuries have the sciences begun to break free from irrelevant presuppositions and to develop autonomously. In the earlier phases of their growth they cannot be studied profitably in isolation from one another or from the doctrines which contemporary

REVIEWS

philosophy imposed upon them. Particularly is this true of astronomy so closely connected in its origins with astrology, alchemy, medicine, mathematics, and other disciplines, and so circumscribed by arbitrary schemes of cosmology.

The task of sketching the early growth of astronomy to the beginning of the seventeenth century in its context of contemporary philosophy and religion, science, and superstition, has now been undertaken by Dr H. C. King, Senior Lecturer in Ophthalmic Optics at Northampton Polytechnic, London, and the author of a standard work on the history of the telescope.

The story opens with some account of the planetary deities and the astronomer-priests of ancient Mesopotamia and Egypt (chs. i and ii). The progress of Greek astronomical thought and technique is next followed from Ionia and the Italy of Pythagoras to Athens and thence to Alexandria and to temporary extinction in medieval superstitions (chs. iii-vii). The fortunes of the Greek scientific heritage are then bound up for some 800 years with the circuitous transmission of Moslem culture (ch. viii), until in the twelfth century the tradition crosses the frontiers of Western Christendom. Following two chapters (ix and x) on the 'gratifying illusions' of anthropocentric cosmology and on medieval enterprises in navigation, cartography, and time-measurement, we reach the fifteenth-century break with the imitative traditions of scholasticism (ch. xi); thereafter the time-honoured ideas of the old astronomy are progressively exploded by Copernicus, Kepler, and Galileo, and the way prepared for the establishment of the modern synthesis (chs. xii and xiii).

Dr King's book does not claim to represent the fruit of extensive original research, being, as he avows, 'derived largely from second-hand sources'. However, he has collected much interesting and significant material from a wide range of reading (his Bibliography comprises 164 titles) and has given us a useful companion volume to more formal presentations of the development of theoretical astronomy. Early man, in the author's view, utilised the vicissitudes of the heavenly bodies to measure the progress of time. 'Yet this temporal need was by no means the only spur to astronomical observation. No less important was his desire to worship something greater than himself, for this found its expression in early fertility cults which often involved particular reference to the moon and later to the entire host of heaven' (p. 2).

We have noted but few slips in this carefully written book. In the reference to the planets on page 36 (l. 9) the word 'major' should read 'superior'. On page 84 (l. 6) 'Cronon' should read 'Conon'. On page 206 (l. 22) 'Ptolemy' should apparently read 'Copernicus'. And the first of the two books attributed to O. Neugebauer on page 245 is the work of A. Neuburger.

A. ARMITAGE

REVIEWS

Reflections of a Physicist. By P. W. Bridgman.

Philosophical Library, New York, 1955. Pp. xvi + 576. \$6.00

THIS book will be welcomed by students of the philosophy of science as it collects thirty-two previously scattered essays by the leader of the once powerful operational philosophy. The papers were published between 1929 and 1954, and they fall into the following headings: operationalism, applications of operationalism to specific scientific problems (mostly physical), sociology of science, and ethics of science. They are all excellently written and, with but a few exceptions, they are accessible to the general scientific reader.

The revolutions in the physics of our century have, like those in the eighteenth century, stimulated the framing of philosophical theories depending on some of the various possible interpretations of certain physical theories. Operationism was one of these philosophies: it had both the advantage of being up to date, and the disadvantage of going the way of all things present—namely, of turning into past. The wide appeal of operationism in the thirties and even in the forties can be understood not only by recalling the prestige of pragmatism in the United States, but also the state of physics itself, which seems to us to have been a lot less sophisticated than it is today. At that time few physicists dared playing with arbitrary cut-offs, or with divergent series, or with Lagrangians involving no 'operationally defined' quantities. Fictions and artificial tricks were thought to belong to classical physics. The new physics knew only 'natural' procedures and, through philosophical autosuggestion, it ended by believing that it employed only symbols designating measurable quantities. Things, properties, and relations that were not defined in terms of possible laboratory operations simply did not exist. (For example, 'A body has position only in so far as its position can be measured', p. 176.) In those days of shrewd innocence relativity used to be explained with the help of an infinite staff of observers filling the universe. This was done on the belief that reference systems, to be physically meaningful, had to have human operators—even if infinite and fictitious, and even if these could not actually observe any Lorentz 'contraction' of lengths. Expositions of the quantum theory often started with Heisenberg's imaginary X-ray microscope for the observation of electrons as if one could dispense with highly elaborate constructions, such as Hamilton's (or Hamilton-Jacobi's) formulation of dynamics. It was also fashionable to state that special relativity had sprung from an epistemological analysis of the simultaneity concept. This was declared physically meaningless even though relativity calculates time differences between inertial systems (which may or may not be inhabited by observers), thereby assigning to simultaneity a non-operational meaning. It was also fashionable to assert that quantum mechanics had

REVIEWS

emerged from an operationist examination of the measurement process, although the quantum theory of measurement was sketched much later (and is still in its infancy), and although ψ was anything but an operationally defined symbol. (It is interesting to note that, unlike many of his followers, Bridgman himself recognised that the operationist requirement had no creative virtue of its own but played only a suggestive rôle in leading to one of the many possible solutions.)

Today, after the enthusiasm for the pragmatic approach to non-pragmatic activities has waned, operationism might be classified as a school of semantics abiding by the empiricist principle that only statements about observations and measurements are meaningful and, consequently, capable of being true or false. Now the theory that 'meanings are operational' (as Bridgman himself describes it) and that scientific concepts must be defined in terms of laboratory operations (to which 'pencil and paper operations' were later on added) is open to at least the following objections. First, both good and bad science contain statements about facts, statements about observation or measurement of facts, and statements about symbols (both with regard to their mutual relations and to their meaning). If only operationally meaningful terms were kept this sandwich would degenerate into a single layer and we would be prevented from realising that observations are observations of something and that many statements are not informative about operations but about formal properties of symbols and about their connotation. Second, science is full of unobserved entities and non-measurable, or at least only indirectly measurable, qualities such as quantity of electricity, energy, potential, entropy, wave phase, adaptation, organisation, organic integration, and so on; they are not, however, occult qualities, but are clearly related to experience by means of theoretical constructions. If we sacrificed all such concepts in the name of some philosophy, we should get rid of the upper, most abstract levels of science, those in which the deepest and most general relations are grasped.

Third, one and the same 'quantity' may be assigned various operational definitions. Thus, according to Bridgman, we must distinguish between a tactual and an optical length. Moreover, some quantities, especially in the most advanced chapters of science, can be defined both operationally and by way of what Margenau calls constitutive definitions, i.e. in terms of further constructs. In the case of temperature if it is defined as that which we 'read' in thermometers (without any underlying theory of heat?) we obtain as many 'definitions' as kinds of thermometers. Even worse, since such 'definitions' presuppose the attainment of an equilibrium state, what then of non-equilibrium situations, which are the most interesting of all? Should we describe them without the benefit of employing the temperature concept just because its operational definition fails for transient phenomena? Is it not one of the advantages of scientific theories that they are

REVIEWS

able to transcend the limitations of the empirical level and that they unify the variety of experience by recognising, for example, that all kinds of thermometers serve the same basic purpose? This only requires us to distinguish qualities and relations from both their intensity and the techniques of their measurement.

Fourth, if meanings are operational, since factual truth is dependent not on form but on meaning, there should be, as Bridgman points out, as many different kinds of truth as techniques employed in establishing the validity of synthetic propositions. (And why not in mathematics as well? Would it not be consistent with operationism to state that there are as many Pythagorean theorems as techniques of demonstration—that is, various dozens?) These different truths would be incommensurable: thus, two different numerical results obtained in the measurement of the length of a stick, one with 'tactual' and the other with 'visual' techniques, would be equally valid. An experimentalist would not be entitled to value one technique higher than another if they relied on different senses.

Fifth, if meanings depend only upon operations, and if operations are, according to Bridgman, essentially personal, then 'we' should end up in solipsism or should at least surrender the communicability characterising (or so we had been told) science as against mystical visions. There is, indeed, an unbridgeable chasm of meaning between you and me, just because our operations are different; as a result, 'science is essentially private'. If so, why care about methodological uniformity—and why be sure that it is possible at all? Sixth, if not only human acts but everything else as well should be analysed into doings (operations) rather than into objects or processes, since operations are private and historically changing, we must conclude that 'nature is intrinsically and in its elements neither understandable nor subject to law' (p. 169). Irrationalism is thus offered as a consolation for the solitude of solipsism: *Thou shalt build the world for thy self, but thou shalt not understand thine own work.*

True, Bridgman retreated early enough from the extreme position he had adopted in *The Logic of Modern Physics* (1927) by granting that 'paper and pencil operations' may be used besides laboratory procedures. They must, if only because the design and actual performance of measurements (not to speak of the building of the apparatus and instruments) requires a host of hypotheses, among them the law statements about the phenomena involved. But then, 'operational definitions' are not a radical starting point but rather, when desirable and possible at all, an intermediate link in a long chain of doings guided by theory about objective facts, human doings, and formal entities. How could such a liberal interpretation of operationism be reconciled with the reduction of concepts to percepts, with the programme of levelling all language strata to the sense-data language (supposing one was spoken among civilised men).

REVIEWS

It is conceivable that operationism—and, in general, pragmatism—could have been fruitful in the social rather than in the physical sciences, not on account of its dislike for speculation (since sociology suffers mostly from poverty of theoretical models) but because of its emphasis on the analysis of phenomena into human doings—which is obviously legitimate (but, then, trivial) in the case of human phenomena. However, in the hands of Bridgman, operationist sociology is just a naïve revival of eighteenth-century social atomism—hence not a science of society at all.

It was important to insist on the need of relating even the remotest scientific (non-formal) terms to experiment but this has been done untiringly since the birth of modern science. It has been less important, and definitely crippling, to strive for the elimination of those terms which are not 'close enough to facts'. It has been equally mistaken to believe that the connection between the various levels of scientific discourse, from sense-data to the most elaborate theories, should amount to a reduction or levelling down. Besides, such (unsuccessful) attempts to reduce scientific theory to the quantitative expression of 'actual facts' had been inaugurated by Mach (whose philosophy Bridgman does not even mention). Indeed, it was Mach¹ who first tried to keep in physics only 'measurable physical attributes', rejecting as 'superfluous and futile' whatever was not reducible to the 'elements' (i.e. the sense-impressions). One wonders why Mach is not credited with the founding of operationism (even granting the *charme* of such 'paper and pencil operations' consisting in the rediscovery of ideas which have been known of old). And one wonders, too, to what level science would have fallen had it been faithful to the operationist programme. General relativity, for example would not have been invented, since it was rejected by Bridgman as un-operational—and rightly so.

Operationism has been an important episode in the philosophy of modern science, and Bridgman's essays are indispensable to its correct appraisal.

MARIO BUNGE

Concepts of Space : The History of Theories of Space in Physics.

By Max Jammer.

Harvard University Press, Cambridge, Mass., 1954. Pp. xviii + 196.

\$3.75

THIS is a systematic and highly stimulating history of the concept of physical space by a scholar who is at home with mathematics, physics, philosophy, theology, and classical languages—a highly exceptional combination indeed. The book's contents are : Foreword by Albert Einstein and Introduction ;

¹ See, e.g. E. Mach, *The Science of Mechanics*, transl. from the 9th edn. by T. J. McCormack, London and La Salle, Ill., 1942, pp. 598 ff.

REVIEWS

(1) The concept of space in Antiquity ; (2) Judeo-Christian ideas about space ; (3) The emancipation of the space concept from Aristotelianism ; (4) The concept of absolute space ; (5) The concept of space in modern science. Philosophers of science will probably be most interested in the last chapter (pp. 125-190).

Like every historical monograph this one assumes principles of historical method and some ideas about the main stages of historical development. The method here employed is the tracing of intellectual influences of certain writers on others, with disregard of the cultural atmosphere in which they lived. The author's basic tenets about the history of culture seem to be : first, that everything worth mentioning began in Greece, and second, that Jewish theological and metaphysical speculation was instrumental in shaping modern science. Thus, pre-classical Antiquity and Indian thought are left behind, and the history of the space concept up to modern times is largely reduced to a strife between Aristotelian and Jewish ideas. Moreover, the thesis is advanced that the Newtonian concept of space—that absolute void functioning as God's sensorium—is entirely or primarily due to Hebrew thought. In support of this latter thesis, Professor Jammer exerts his ingenuity in devising a causal chain starting from eminent Rabbis in the Hellenistic period and culminating in Crescas (c. 1400), the Catalan Jewish schoolman who criticised Aristotelianism from the viewpoint of Hebrew theology. Not the Greek atomists, who are barely mentioned, not Giordano Bruno, but Crescas is credited with presenting the West with the opinion that physical space is void, homogeneous, isotropic, and infinite ; moreover, Crescas' thought would have influenced Newton's world view even more than his own physical research, the revival of atomism, the geographical discoveries, and Renaissance pantheism. Much in the same vein, the fugacious revival of optics during the thirteenth century is regarded by Jammer as a mere result of the neo-Platonian thesis that space is light and God's emanation. But is this consistent with the decline of neo-Platonism just at that time ? And even assuming that such a renaissance was nothing but a miraculous consequence of neo-Platonian writings why did these same writings lack such a power in Byzantium ? The reviewer's main objection to this kind of history of ideas is that every theory is thereby regarded as functioning in a cultural vacuum. Actual life, the epoch's interests and strivings, its *Zeitgeist* are ignored. Thinkers are treated like bookworms obtaining their whole nourishment from written paper, which nourishment barely enables them to produce further paper to feed further worms. In such a way it is not explained why author A_n was influenced precisely by author A_{n-1} and succeeded in influencing, in his turn, author A_{n+1} .

Although it is one of the author's merits to have compressed a wealth of material in a few pages, some omissions are worth while correcting in a future edition. For example, Euclid's ideas on physical space and on

REVIEWS

geometry (which he conceived as the science of the ultimate nature of matter) ; Augustine's thesis that space and time are inseparable from matter and change ; Oresme's reinvention or rediscovery of co-ordinate geometry ; and the contemporary idea that microphysical space is a fluctuating jelly ($ds = \gamma_\mu dx^\mu$). On the other hand, some feebly grounded speculations like those of the 'new' cosmology should not be treated on a par with theories that have stood some empirical test.

There are also, of course, quite a few debatable assertions. Thus, Plato is credited with the geometrisation of physics. How could he if he denied the very possibility of building a science of nature ? Besides, Aristotle's cosmos was not thoroughly ordered : the sublunar sphere, man's home, was chaotic and corruptible. And how could Al-Ghazali be regarded as the typical representative of Muslim philosophy unless it were previously demonstrated that there has been a sufficiently homogeneous system of 'Muslim philosophy' ? Also, has recent criticism not discredited Duhem's rash assertion that Philoponos' *impetus* theory was the starting point of modern dynamics (instead of being an unsuccessful attempt to patch up peripatetic physics) ? Further, why should Mach, who rejected the relativity theory, continue to be regarded as a forerunner of relativity, and not as a thinker whose criticism of Newtonian mechanics contributed to defrost the attitude towards dynamics ? Finally, it is controvertible whether logical positivism can be credited with the recognition of the physical implications of modern theories of space. Is not the structure of physical space according to most positivists (including Jammer) 'a function of our conceptual scheme' ? Partisans of relativity, on the other hand, tend to think, as Einstein himself recalls it in his Foreword to this book, that 'there is no space without a field' and that space is not independent of matter (whether in its particle or in its field form). Moreover, if special relativity reinstated Leibniz's relational theory of space (by regarding spacetime as a system of objective relations among point events), general relativity has superimposed on this relational structure the Cartesian concept of space as a thing (by welding space and gravitational field to such a point that a field-free region is not regarded as being *in* space).

Despite many controvertible points, Jammer's book is a *must* in its field, not only because of its impressive amount of information, but mainly because it is anything but a catalogue of ideas and books. It is a serious examination of the problems, scientific, philosophical and theological, clustered around the space category. The inner logic of theories (but unfortunately not their cultural background) is discussed with deepness and clarity. The present reviewer therefore hopes that Jammer's book will elicit what his philosophy of history seems to preclude : namely, the emergence of new ideas.

MARIO BUNGE

REVIEWS

Les Fondements logiques des mathématiques. By E. W. Beth.
Gauthier-Villars, Paris, 1955. Pp. xv + 241.

THIS is the second edition, revised and enlarged, of a book that first appeared in 1950. It is presented as an introduction to the methods used in the foundations of mathematics (p. xi), a survey of contemporary views in this subject, and an explanation of their philosophical consequences (p. 5).

The range of topics covered is very large. We are offered summary accounts of mathematical logic (propositional calculus and functional calculus), set theory, the customary recursive definitions of real and rational numbers in terms of integers, an outline of syntax and semantics (with special emphasis on methods developed by Gödel and Tarski), the Frege-Russell reduction of arithmetic to logic, the views of intuitionism, an extensive discussion of the logical antinomies and much else. Most of the important work of the past century is at least mentioned, and there are scattered references to investigations no more than five years old.

The 'philosophical consequences' are briefly discussed in a preliminary chapter on Aristotle's conception of science, and in a concluding section. The author's view is, briefly, that western philosophy and science have been dominated by a rationalistic ideal first authoritatively stated by Aristotle. It is the pattern of Euclid's *Elements*—an organised body of knowledge, arranged in the form of theorems connected by chains of proofs with explicitly formulated axioms, themselves incapable of proof. Traditional rationalism, still powerful today, invokes logical and metaphysical principles, held to be self-evident, indemonstrable, and certain. Now the development of the philosophy of mathematics (and especially the discovery of the logico-mathematical paradoxes) has forced a radical revision of the supposedly self-evident principles of logic and mathematics, though not in any fashion that commands universal agreement. It is not clear what moral is intended to be drawn, unless it is that it is harder than ever to be a rationalist in these days.

The subject of the 'foundations of mathematics', largely ignoring the philosophical interests of its pioneers, has become a vast domain of highly technical studies, in which the non-specialist badly needs the orientation which an introductory survey might be expected to provide. The outsider need only look at G. Kreisel's stimulating paper, partly intended for 'the general reader' (this *Journal*, 4, 107-129), to appreciate the difficulties of access. And the original papers are still more forbidding, as a glance at any volume of the *Journal of Symbolic Logic* will confirm.

Yet it is possible to provide the kind of help that is needed. Among the examples that might be cited are Russell's *Introduction to Mathematical Philosophy*, the introductory chapters of *Principia Mathematica*, W. V. Quine's essay on Whitehead (in *The Philosophy of Alfred North Whitehead*, Evanston,

REVIEWS

1941), Alfred Tarski's 'The Semantic Conception of Truth' (*Philosophy and Phenomenological Research*, 4, 1944, 341-375) and Raymond L. Wilder's *Introduction to the Foundations of Mathematics* (New York and London, 1952). The kind of 'popularisation' achieved in these surveys demands very special gifts—among them a capacity to disentangle the 'point' of a mathematical or logical proof without pretending to reproduce the full argument or blurring the line between what can be understood immediately and what has to be taken on trust. The reader must get immediate insight, in full awareness of the extent of his ignorance and the measures needed to repair it.

It would be welcome news if the book under discussion had met these exacting standards. Such results as the various Gödel theorems, the Löwenheim-Skolem theorem, modern investigations on models, and the stimulating ideas of the intuitionists deserve to be widely understood. But this book cannot be recommended for non-specialists: its explanations are often obscure, sometimes inaccurate, and hardly ever successful in communicating 'grasp' or 'insight'. A beginner would be bewildered by finding trivialities jostling remarks pre-supposing an elaborate mathematical education; and the expert must be repeatedly irritated by vagueness or imprecision. It is hard to forgive a writer who gives references by citing only the author's name and the year of publication. The bibliography provided is inadequate.

The book reads like a set of notes to be used in connection with a course of lectures. It would need very thorough revision and expansion to fulfil the task to which it addresses itself.

MAX BLACK

Prediction Methods in Relation to Borstal Training. By Hermann Mannheim and L. T. Wilkins.

H. M. Stationery Office, London, 1955. Pp. vi + 276. 17s. 6d.

THIS is a highly competent piece of work. It sets out to make a contribution to methodology—not in the sense found in philosophy of science but in the natural sense of that word; that is to say, it is a study of methods of doing something such as playing golf or playing chess—in this case predicting proportions of successes and failures among the boys (whose technical name is 'lads') selected for Borstal. The authors, imposing certain conditions on themselves—the method must be simple, efficient, repeatable, and 'valid'—hope to provide something that is usable by the Courts and that will be an improvement on previous prediction methods. In their aims they appear to be entirely successful; and it is pleasant to find statistics used in this field for sensible purposes. It makes, subject to one important reserve to be mentioned later, an important contribution.

REVIEWS

The problem is this. A boy is considered for Borstal. A decision has to be taken between two possibilities—putting him in Borstal or not doing so. If put in, then on release he will either get into further trouble and return to Borstal or he will not. If he returns he is a 'failure', otherwise a 'success'. How can we predict of possible candidates for Borstal which ones are likely to be successes and which ones failures in this sense?

There are two general ways of setting about this: compile all conceivably relevant factors (such as drunkenness, broken home, other delinquents in family); score existing Borstal boys for these factors; and one can then obtain a table and scores for possible Borstal boys. The other method, which is followed by the authors, is simpler: weight the factors; omit those that statistically do not influence the result; and one can arrive at a table more simply and quickly. It then has to be validated for other samples of boys.

The authors call this a prediction table, warning us that it is in fact something else—an 'experience' table, used for prediction. A better name might be 'situation-table' or 'situation-index'. Further developments are possible. This index can separate definite sheep from clear-cut goats; but there is a nondescript residue. Still, if the foregoing method is one of rough tuning, another for fine tuning can be utilised for the residue.

As might be expected, these methods predict better than do medical reports, assessments by prison officials, etc. An unexpected result is this. The fact that 'open' Borstals are significantly more successful than 'closed' ones might be attributed to their being allotted boys who are better material and therefore a better risk. But by means of statistical procedures the authors have been able to show that, while this is in part the case, it is not sufficient to explain the greater success of the open Borstal, and that this must be credited with a greater reforming influence.

I turn now from the content to the form of enquiry.

The authors labour somewhat at the justification of their way of working. Thus from the fact that their method can predict successfully in seven cases out of eight some critic is supposed to say that therefore one case is incorrectly classified, and the authors explain that this is a mistake. It is hard to understand how anyone who is capable of reading this book seriously could make this mistake. Conceivably this and other comments are intended for dynamic psychologists who think that, because work like the present has nothing to do with the inner lives of the boys, therefore it cannot be of any use at all.

The schism—investigating unconscious conflict versus investigating other matters—is a curious one. An insurance company can tell what proportion of people is likely to die by the age of forty-one without knowing anything about their health or medical history. Likewise it is reasonable to try to calculate how many will be recidivists without knowing anything about their mental make-up.

REVIEWS

On the other hand it does not follow that, because a statistical inference of this kind is valid, there is therefore nothing for the dynamic psychologist to add. The authors make it plain that there is something—and it is especially interesting to see what this is. After the table or index, with rough and fine tuning, has yielded all it is capable of, the result may be, say, that you have isolated a large group of boys with a 7 to 1 chance of success if they go to Borstal—but you don't know which are which (in this connection it is interesting that the rough tuner predicts on the basis of information about past criminal career, whereas the fine tuner does so on the basis of personal qualities). To try to find out which are which one could use interviews or methods of dynamic psychology (subject to a certain proviso). This means that, though neither interviewing in general nor psychiatric investigation in particular is uniformly satisfactory for prediction of recidivism, the interviewer or the psychiatrist could be on very strong ground if, after making his prognoses and classifying them as 'successes' or 'failures', he then modified them to make them fit the proper statistical proportions—a procedure often adopted by psychologists and by examiners in distributing marks.

The proviso stipulated by the authors is that the interview should utilise factors other than those used to form the statistical tables (otherwise it would not be independent of them but merely a repetition and a poor one). This condition might or might not be fulfilled if the interviewers were well-meaning people without special training; it would almost certainly be fulfilled if the interviews were conducted by clinical psychologists, for such an interviewer would be using as factors the impact of the boy on himself in certain specific ways, the boy's particular way of putting things, and his mode of responding. Thus the proviso may be fulfilled in practice. But the authors have surely made a serious theoretical mistake in supposing that there is any need to fulfil the proviso at all. Take the set of factors required to form the index and give them without any other information whatever to a clinician, and he will bring to bear on them a background of experience about their significance and how they can fit together. The authors' assumption would be true only if their statistical procedure could squeeze all possible information out of the given factors—and this no one could claim.

This leads to a further interesting point made in the book. It is held, with regard to 'intangible' factors, such as those a clinical psychologist deals with, that they are amenable to scientific tests provided a form of words can be agreed that makes the intangibles communicable. This is arresting, mystifying, and would have been worth elaborating.

To sum up the methodological position: A new type of statistical procedure is being developed, which shows considerable promise of yielding fairly accurate predictions. That such a procedure is possible is of

REVIEWS

theoretical interest, especially as the factors used in predicting are not causal. The method of the present book has likenesses to forecasting in meteorology. You have a thermometer, a barometer, and so on. Changes of temperature, pressure, humidity, and cloudiness are not necessarily causes of change in the weather ; but whether causal or not they are indices of the change. A clear example of a good predictive index would be the clinical signs of rising temperature and falling pulse ; yet neither would cause the ensuing state of the patient—the causes would be quite different. In other words, this book provides a dashboard with a number of dials which enable you to know where you were and are and where you are likely to get to. That is why I have suggested that 'index' would be a good name for the tables, for it would convey clearly both their rôle and value and also their limitations ; that is to say they will tell you what may become of a Borstal boy, but they tell you nothing about him, and of course can be no guide about what is a wise way of handling him. The 'intangibles' despite their intangibility become indispensable at this point. Despite the limited scope of the method, it is a triumph of statistics to be able to use so successfully such relatively superficial quantities as indices. Their practical importance is that they can be used when the actual causal factors are too complex to unravel ; their theoretical interest is that they can function in this way at all, seeing that they are not linked in any simple way to the causal factors.

A final word on book-making. The authors bring out the complementary rôle of this statistical work and of clinical psychology but do not bring it out strongly enough—readers might easily, though wrongly, get the impression that the methods described in this book were sufficient (or the main ones) in deciding questions about delinquents, and busy Home Office officials, whose policy may be shaped by this work, might well fall victim to this impression—which could become serious. A point of architecture : the admirable history of the subject is, as is most often the case, in the wrong place as chapter I, where it tends to put one off. The time for history is after the problem has been outlined—then its interest becomes obvious at once. A small point is the repeated reference by the authors to an equation that is never given. Finally could the publishers not produce the book in a manner more worthy of their patron ? Such paper and lay-out put one off reading it—after all, this is a Government production that was probably meant to be read.

J. O. WISDOM

Factor T. By Stefan Themerson.

Gaberbochus Press, London, 1956. Pp. 68. 6s.

THIS is a brilliant little essay, evidently written by a kind of mind that is all too rare. *Factor T* is the tragedy that results from Needs plus Dislike of

REVIEWS

what is needed—when it is vitally necessary to do what one vitally dislikes. This is shown in the Dislike of tomatoes, without which life could not be supported, among a tribe in Notwahrocteh. A negative form of the factor T at the moment of writing is that this book is outside the sphere of reviews in this *Journal*, yet it is one that especially philosophers of science should enjoy. Or does the author's adventure with the problem of induction alter matters? The solution offered is not so striking but it abounds in excellent sidelights.

The quality of the writing is delightful, the author clear and cogent in logic and psychology. He deals faithfully with the inference :

Only an elephant or a whale gives birth to a creature whose weight is 70 kilogrammes or more ; the President's weight is 75 kilogrammes ; therefore the President's mother was either an elephant or a whale.

And the essay may be said to contain a fundamental theory of psychology having affinities with Freud and Plato. These are good chromosomes to be endowed with, especially when grown in a Lewis Carroll environment. The book is excellently illustrated by Franciska Themerson.

J. O. WISDOM

La Théorie harmonique. Vol. I : *Le Principe de simplicité dans les mathématiques et dans les sciences physiques* ; Vol. II : *Biologie*. By André Lamouche. Paris, Gauthier-Villars, 1955, 1956. Pp. 481 + 575. 3,000 fr.

THIS monumental work is the product of thirty years of study in the history and methods of the sciences on the part of a well-known French engineer. The reviewer cannot claim to have fully digested all the arguments which are here presented with great lucidity, subtlety, and scholarship, but wishes to call attention to these volumes and to the author whose *Méthode générale des sciences pures et appliquées* (Gauthier-Villars, 1924) received little notice in the English world.

The *Harmonic Theory* is a comprehensive neo-Pythagorean interpretation of the characteristics both of natural thought and of rational processes, having as one of its ultimate aims a strengthened harmony of thought and action. The interpretation is based on a *principle of simplicity* which is applied to mathematics, physics, and biology. Under this principle the parallel development of complex from simpler logical structures, and of complex from simpler natural phenomena, involves chains of steps determined by a *rule of maximum relative simplicity*. The master concept of *simplicity* is subjected to a close analysis, and the ultimate exemplar of simplicity is found in the integral numbers. It is for this reason that Lamouche's philosophy of

REVIEWS

an underlying harmony between thought processes and other natural phenomena has been called 'a true pan-Pythagoreanism'.

Much reading and thought have gone to make this eloquent salute to a persuasive simplicity, and insight is displayed in the avoidance of some current errors. For example, following Planck, *a priori* metaphysics and radical positivism are treated as twin enemies to the growing seeds of thought. This openness towards new developments may result from the fact that the author's intention is to discover a principle which is so deeply implicit in the use of reason as necessarily to cover simultaneously all applied logic and all scientific theories.

This high generality can be a source of weakness. One suspects that all facts which can be conceived and rationally ordered could in some manner be read as supporting the author's doctrine. In that case the doctrine may be an intuitive anticipation of one feature of a future scientific philosophy covering all phenomena, including thought. But if that is so, then the author (like all of us) still lacks certain indispensable insights which could enable his discursive treatment to be converted into a concise and unmistakably authentic and objective statement of valid principles underlying all systematic knowledge.

It is impossible here to discuss the author's arguments in detail. The volume on physics is concerned with such topics as simplicity, rhythm, unified fields, complexity, discontinuity, and determinism; the volume on biology with chance and finality, unity and diversity, levels of organisation, selectivity, specificity, and exploits the conceptions of the finality and 'inventiveness' of organic processes which have been stressed by Cuenot. There is a bibliography, but no index.

One section is headed 'Aimez qu'on vous critique'. This encourages the following comments. If a Pythagorean philosophy is sufficient for science, why are so many basic problems still obscure? Can asymmetrical relations ever be adequately treated by a doctrine based primarily on equivalences? If a principle of simplicity is all-comprehensive and already correctly understood, why are a thousand pages necessary?

But every sensitive reader must admire an author with so many talents.

L. L. WHYTE

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(a) BOOKS RECEIVED FOR REVIEW

- Bender, William, *An Introduction to Scale Coordinate Physics*, Burgess Publishing Co., Minneapolis, 1958, pp. ix + 340, \$7.50
- Birr, K., *Pioneering in Industrial Research*, Public Affairs Press, 1957, pp. vii + 204, \$4.50
- Cannon, H. Graham, *The Evolution of Living Things*, Manchester University Press, 1958, pp. ix + 180, 12s. 6d.
- Cassirer, E., *The Philosophy of Symbolic Forms*, Vol. 3, *The Phenomenology of Logic*, Yale University Press, 1958, pp. xvii + 501, £3 8s.
- Cornu, A., *The Origins of Marxian Thought*, Charles Thomas, Illinois, 1957, pp. 128, \$3.75
- Durable, Le R. P., *Initiation à la logique*, Gauthier-Villars, Paris, 1957, pp. 89, Fr. 1,400
- Ferreira, Carlos Vaz, *Moral para Intelectuales*, Universidad Nacional de la Plata, 1957, pp. 263
- Freeman, T., Cameron, J. L., McGhie, A., *Chronic Schizophrenia*, Tavistock Publications Ltd., 1958, pp. x + 158, 21s.
- Hallett, H. F., *Benedict de Spinoza*, Athlone Press, London, 1957, pp. xvi + 171, £1 5s.
- Jones, E. J., *Sigmund Freud Life and Work: The Last Phase 1919-1939*, Hogarth Press, London, pp. 536, £1 15s.
- Kattsoff, Louis O., *Physical Science and Physical Reality*, Martinus Nijhoff, The Hague, 1957, pp. 311, £1 15s. 6d.
- Koopmans, T. C., *The State of Economic Science*, McGraw-Hill Publishing Co., 1957, pp. xi + 231, £2 9s.
- Korner, S. (Ed), *Observation and Interpretation*, Butterworth's Scientific Publications, 1957, pp. xvi + 218, £2
- Ladrière, Jean, *Les Limitations internes des formalismes*, Gauthier-Villars, Paris, 1958, pp. xiii + 715, Fr. 650
- Lamouche, A., *L'Homme dans L'Harmonie universelle*, La Colombe, Paris, 1958, pp. 242, Fr. 980
- Mukerji, A. C., *Self, Thought and Reality*, 2nd edn., Indian Press (Publications) Private Ltd., Allahabad, 1957, pp. xx + 437, Rs. 10
- Pasquonelli, A., *Introduzione alla logica simbolica*, Edizioni Scientifiche Einaudi, 1957, pp. x + 118, L. 1200
- Popper, Karl R., *The Poverty of Historicism*, Routledge and Kegan Paul, London, 1957, pp. xiv + 166, 16s.
- Peirce Charles S., *Essays on the Philosophy of Science*, Liberal Arts Press, 1958, pp. xxii + 271, \$1.00

RECENT PUBLICATIONS

- Samuel, Viscount, *In Search of Reality*, Blackwell, Oxford, 1957, pp. 229, £1 8s. 6d.
- Schnapper, M. B. (Ed), *New Frontiers of Knowledge*, Public Affairs Press, Washington, 1957, pp. x + 125, \$2.75
- Singer, I., *Santayana's Aesthetics*, Harvard University Press, London: Oxford University Press, 1957, pp. x + 235, £1 18s.
- Stegmüller, Wolfgang, *Das Wahrheitsproblem und die Idee der Semantik*, Springer-Verlag, Vienna, 1957, pp. viii + 328, £2 16s.
- Taton, R., *Reason and Chance in Scientific Discovery*, Hutchison, London, 1957, pp. 171, £1 10s.
- Tramer, M., *Philosophie des Schöpferischen*, Francke Verlag, Bern, 1957, pp. 87, \$8.80
- Waddington, C. H., *The Strategy of the Genes*, George Allen and Unwin, London, 1958, pp. 262, £1 8s.
- Wiener, Philip P. (Ed), *Roots of Scientific Thought*, Basic Books, New York, 1957, pp. x + 677, \$8.00

(b) ARTICLES

- W. Ackermann, 'Philosophical observations on Mathematical Logic and on investigations into the Foundations of Mathematics', *Ratio*, 1957, 1
- G. Builder, 'The Resolution of the Clock Paradox', *Australian Journal of Physics*, 1957, 10, 246-262
- Juan Comas, 'El Proceso Filogenetico Humano', *Cuadernos del Seminario de Problemas Científicos y Filosóficos*, 1957, 11, 53-93
- C. K. Davenport, 'El Papel de Los Metodos Graficos en la Historia de la Logica', *Suplementos del Seminario de Problemas Científicos y Filosóficos*, 1957, S₃-41-65
- Philipp Frank, 'El Origen de la Separación Entre las Ciencia y la Filosofia', *Suplementos del Seminario de Problemas Científicos y Filosóficos*, 1957, S₁-1-26
- Philipp Frank, 'Las Razones Para Aceptar Las Teorias Científicas', *Cuadernos del Seminario de Problemas Científicos y Filosóficos*, 1957, C₁-1-14
- Roger Garaudy, 'Del Empirismo Logico a la Semantica', *Suplementos del Seminario de Problemas Científicos y Filosóficos*, 1957, 11, 35-51
- A. Grunebaum, 'I. Complementarity in Quantum Physics and its Philosophical Generalization', *Journal of Philosophy*, 1957, 54, No. 23, 713-727
- E. Gross, 'Toward a rationale for science', *Journal of Philosophy*, 1957, 54, 713-727
- J. A. Laso, 'Esquema de una Filosofia de las Matematicas', *Cuadernos del Seminario de Problemas Científicos y Filosóficos*, 1957, 11, 21-33
- G. H. Muller, 'On the Operational Foundations of Logic and Mathematics', *Ratio*, 1957, 1
- K. R. Popper, 'The Aim of Science', *Ratio*, 1957, 1, 24-35
- J. Rebersat, 'La Conciencia Ibero Americana y Sus Problemas', *Suplementos de Seminario de Problemas Científicos y Filosóficos*, 1957, S₂-27-39